RECORDS

OF

GENERAL SCIENCE,

BY

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OF

GENERAL SCIENCE.

ARTICLE I.

*Life of the Rev. John Flamsteed, First Astronomer-Royal.
Written by himself.*[*]
(Concluded from vol. ii. p. 341.)

BUT Mr. Newton was not displeased with their flattery;
nor ever (that I could hear of) endeavoured to correct them.
We conversed civilly as oft as we met accidentally: and
he failed not (as if he were a great master of my methods)
always to ask " how the catalogue went on?" To which I
always gave sincere answers; telling him how far I had
proceeded, and that I wanted more hands, both to carry on
the observations and calculations that were necessary. But

[*] It appears unnecessary to apologize to the reader for occupying so many of our
pages with one of the most interesting documents which has appeared during the
present century. The biography of Flamsteed being the property of the country,
it is only the duty of a journalist to call the attention of the public to it, for the
purpose of removing calumnies which have unjustly cast a gloom over the character
of an illustrious philosopher. To Mr. Baily the memory of Flamsteed owes an
incalculable debt of gratitude,—not only for the labour which, as editor, he has
bestowed upon the work to which we are indebted for our extracts, but for the
minuteness of his investigations with regard to Flamsteed's character. This is
particularly exemplified in his exposure of a most extraordinary error committed
by Sir David Brewster, in his Life of Sir Isaac Newton, which calls for immediate
correction at the hand of the biographer himself: at page 242 of his Life of Newton,
a very coarse, ill-natured letter is given, with the signature of Flamsteed; while
the original, which we believe to be in the University of Oxford, and to have been
seen by Sir David, has the name of Sir Isaac Newton attached to it! By what
authority was this alteration made?—EDIT.

this I could not get him to take notice of. In the mean-time, some friend of mine (that was frequently in company with me, and saw how the work went on, with such assist-ance as I hired and paid myself, and was informed what the charge would be of printing the observations of thirty years, and engraving the maps of the constellations I had prepared), acquainted Prince George of Denmark with my performances. Mr. Newton lived near the Court : I always at a distance. He was the president of the Royal Society, and had a great courtier as his friend, and one who was frequently at his office, required at court, and attending on the Prince. So that he could not but hear of the Prince's inclination to make me easier in my work; nor could Mr. Newton fail to be informed of it. So, on the 10th of April, 1704, he came down to Greenwich, visited me on my request, stayed and dined with me. At his first coming he desired to see what I had ready for the press. I showed him the books of observations, together with so much of the cata-logue as was then finished (which was about half), and a fair copy of it, with the maps of the constellations drawn, both by my amanuensis and Vansomer. Which having looked over carefully, he desired me to let him have the recommending of them to the Prince. I was surprised at this proposition. I had formerly tried his temper, and always found him insidious, ambitious, and excessively covetous of praise and impatient of contradiction. I had taken notice of some faults in the fourth book of his *Prin-cipia,* which, instead of thanking me for, he resented ill. Yet was (so) presumptuous that he sometimes dared to ask " Why I did not hold my tongue?" I considered that if I granted what he desired, I should put myself wholly into his power, or be at his mercy, who might spoil all that came into his hands, or put me to unnecessary trouble and vexa-tion about my own labours; and all the while pretend that he did it to amend faults where none were, but were un-avoidable, or easy to be corrected, and, therefore, excusable. I had further irritated him by not concealing some truths that are since published in print, and notoriously known : and, therefore, civilly refused what he desired. But still he told me he would recommend them to the Prince, and

parted with me in the evening, with a short expression of very good advice. " Do all the good in your power," which it would have been very happy for him if he had followed himself, and has been the rule of my life from my infancy.

But I heard no more of his recommendations. On the contrary, his flatterers, and such small mathematicians about London as hoped to get themselves esteemed very skilful, even by crying up his book, began to ask " Why I did not print?" As if I were obliged to publish my works just when they pleased, though they understood no more of my works than they did of his books, which they so much cried up. To obviate this clamour, I examined all my books of observations, and took an account of what number of folio pages they might fill when printed, and found it much greater than I expected, Whereupon I drew my *Estimate* into a short paper, wherein I both showed what the number of pages were, and also in what order the press was to work them off. And chiefly urged that the maps of the constellations would be first of all set upon : that being carried on apart, they might be finished by the time the observations were printed off.

Vansomer, an excellent designer, who had drawn about a dozen figures for me, was then alive, and ready to go on with the rest. My amanuensis had not left me, and might have been hired again to continue in my service : Mr. Hodgson's help might also have been purchased. Some of my acquaintance had fallen into a suspicion that my labours answered not what might reasonably be expected from me. That I might cure them of these misapprehensions, which had been impressed upon them by the false and malicious suggestions of some few arrogant and self-designing people. I gave a copy or two of his Estimate to an acquaintance of mine, desiring him to shew it to those of my friends who had been possessed with these unjust suspicions. At one of the meetings of the Royal Society some of them were present : he got my paper handed to one of them, who sat at a distance, (for their meetings were thronged with company, however thin they are at present), who, opening the paper and finding the contents, delivered it to the Secretary, who read it at the board. This convinced the members present that I had been unjustly aspersed ; and it was moved that

the printing of the whole should be recommended to Prince
George by the Society.

Accordingly a committee was appointed, who, with Mr.
Newton, waited on the Prince. But who they were when
they waited on him, and how they made their recommenda-
tion, I was never informed : nor did they vouchsafe to con-
sult me about it, or take me along with them. All that I
can tell of it is that the estimate was wrote in November,
1704; the Prince chosen into the Society November 30th :
a letter from the Prince's secretary, Mr. George Clarke,
directing Mr. Roberts, Sir C. Wren, Dr. Gregory, and Dr.
Arbuthnot, with Mr. Newton, to inspect my papers, dated
December 11th 1704, which they did, and sometime after
gave in their report of the charge of preparing and printing
the observations and catalogue, mentioned in the estimate
about £863, viz. 283 reams of paper for 400 copies, at 20s.
per ream, £283; composition and press-work for 300 sheets,
at 20s. per ream, £300; charges of an amanuensis for
copying ditto, £100 ; to compute the planets' places for two
calculators, £180. But the last particular of the charge
(£180 for two calculators) was not mentioned *in* it, but
added in a note *under* it; for what reason those know best
who drew it up. Nor the charge of designing and engraving
about 50 plates of the constellations : though this was likely
to be the heaviest part of the charge, and the observations
could not be understood without them. I had further pro-
posed them to be the first taken care of and begun. I had
them all drawn ; and twelve of them anew designed by a
skilful workman, by me. These were the most sumptuous
parts of the work ; and, had it not been for them, I had no
or little need to crave the Prince's help to print. Why
they were neglected, Sir Isaac Newton best knows. Betwixt
March 22, 1704-5, and April 21, 1705, Mr. Newton was
knighted by the Queen, at Cambridge, (April 21st 1705.)

Whereby I was plainly convinced that Sir Isaac Newton
was no friend to (my) work ; and every step he took after-
wards, proved plainly that, whatever he pretended, his design
was either to gain the honour of all my pains to himself,
to make me come under him, (as Dr. Arbuthnot sometime
after expressed), or to spoil and sink it : which it was my
chief concern and business, if possible, to prevent. I, there-

fore, printed my estimate and gave it to my friends, that they might see what my works were, and how I thought it best to proceed in printing them.

To screen himself from the just imputation and blame that would probably follow such disingenuous and ungrateful practices, he made use of these gentlemen, to whom he had got the inspection of my book of observations ordered by the Prince, and called them the Prince's referees. Of these, Sir C. Wren was then about 70 years of age; and though he was a skilful person, yet being full of other business, he was sure to have him, who lived in the neighbourhood to consent to all his orders, but knew little of the business. Mr. Aston had been fellow of the same [Trinity] College in Cambridge, at the same time with him, knew nothing of the business, lived in the Court, had been my friend and guest at the Observatory, was too much a courtier to withstand any one that had a noble patron in the ministry, and therefore, was taken into the number of referees, sometimes for special purposes. Dr. Gregory, though he published a piece of astronomy, knew but very little of that part of it that was cultivated here. Nor was Dr. Arbuthnot skilled in it; but being one of the Prince's physicians, he was taken in to serve Sir Isaac Newton's purposes. He saw what was designed, and testified to me by some expressions, that he approved not such proceedings; promised once to assist me in a particular affair; and though he met with obstructions, performed it handsomely.

With these persons, Sir Isaac Newton began to act his part and carry on his designs. I deal honestly and openly with him, as will appear by the copies of some letters I wrote to him upon several occasions; having no other design but to have my work handsomely printed, and as soon as possible; for the Prince was very infirm. But I soon perceived, that he designed only to hinder the work by delays, or spoil, or sink it, or force me to comply with his humour, and flatter him, and cry him up as Dr. Gregory and Dr. Halley did. I was forced, therefore, to act with more caution than I had done hitherto, that I might give him no cause of pretensions to stop the progress of the work, to forward which, I used my best diligence and honest endeavours. I hired one, and employed him to copy specimens

of the several parts of the work, but could not get them printed off, till March 22nd, 1704. In the mean time, Sir Isaac Newton appointed a meeting of his referees, March 5 following, Mr. Churchill was not there; but Sir Isaac, with Dr. Arbuthnot, Dr. Gregory, and Mr. Aston, dined at Churchill's; and a fortnight after, Mr. Aston told me of it (for I dined not with them), and that all things were then agreed but paper. Now, I understood that Mr. Churchill was to be the undertaker ; he had been recommended for that purpose, by one that I took to be my friend, without my knowledge, for I did not conceive that we had any need of one, and so did some of the gentlemen of the society. But, Sir Isaac Newton was resolved to make friends at my cost. For, as he ordered the matter, the undertaker was here to reap the sole advantage of all my labours and great expenses : and he was so confident of it, that when I intimated it to him, he answered boldly, " The Prince would reward me for them," however, there was no receding ; for then Sir Isaac Newton's criers up, would have clamoured, " that I hindered the printing of my own works myself." To avoid that imputation I was silent, though I complained oft to some friends in private, but never did any thing whereby it might appear I allowed him. At this meeting, on the 5th of March, the specimens of the undertaker's printing were produced, but found to be ill done. I got others done very well, and paid the printers myself.

June the 11th following, Dr. Gregory and myself, with Mr. Churchill, dined at Sir Isaac Newton's, when they agreed to give Mr. Churchill £1. 14s. per sheet. They signed the agreement, but I would not, although they urged me much, I desired to be excused ; for it was plain to me, that he designed not the good of the impression or my advantage, but to make him a friend of a great name, by obliging a person I never had any acquaintance with, and enriching him at my cost. This point being over, I was in hopes that the press should have been set to work immediately; for I had about 50 sheets of observations made with the sextant ready copied, and the rest of that sort would easily be finished before these could be printed off. But I found myself deceived; we were as far off from printing, as if no such bargain had been made.

At midsummer following, I paid my amanuensis and calculators a quarter's pay myself; and, Sir Isaac Newton, to encourage me to do it, talked often of drawing the Prince's money. But, when I waited on him, July 4th following, and told him that I must go into Surrey to reap my harvest, (as usually I did every year about this time), he put me off again, before I could say any thing to him of it, by telling me that Dr. Arbuthnot's daughter was so very ill, that the Dr. could do nothing till her recovery; and, that it was not fit we should begin to print till we had received his Royal Highnesses money; and that it would be soon enough at my return. I had put 12 sheets, ready for the press into his hands a week before. He thought to work me to his ends, by putting me to extraordinary charges in maintaining and paying an amanuensis and calculators myself, at my own charges. But I resolved to bear the expense patiently and defeat his designs. After this, I caused my amanuensis and calculators to go on with their work, and carried on the observations for completing the catalogue and others, according as I had opportunity. But, Sir Isaac became daily more perverse, and sought by several vexatious pretences to discourage me and weary me if possible. I paid my calculators and amanuensis three quarters, without any present prospect of being any way reimbursed. But yet I had hopes, if once the press began to work, they would not find any new tricks or pretences to delay re-paying me. But herein too, I found myself mistaken : those that have begun to do ill things, never blush to do worse and worse to screen themselves. Sir Isaac Newton had still more to do, and was ready at coining new excuses and pretexts to cover his disingenuous and malicious practices. I had none but very honest and honourable designs in my mind; I met his cunning forecasts with sincere and honest answers, and thereby frustrated not a few of his malicious designs. Finding that I persisted unwearied in my purposes, he demanded to have my *First Night Notes* put into his hands, that he might compare them with my copy. These were wrote in quarto volumes, and from them were commonly transcribed correctly into large folios, next morning, from which the copies were taken. I knew that he would be mistaken, and that they would

not serve his design; · they were put into his hands, February 23rd, 1705-6.   Mr. Hodgson acquainted me that Sir Isaac had showed him three or four pages of errata, that were committed in transcribing as he supposed : and a table made by Dr. Gregory for turning the revolves of the screw into degrees, minutes, and seconds, wherein he *wisely* had supposed the screw every where equal and equable.   I smiled at this, and promised to send them my own tables for that purpose, and showed them their mistakes and that there were no material errors committed. This was some small mortification to them, but they had learned not to be ashamed.

Though I had refused to handle any of the Prince's money but what was to pay my proper disbursements, and Sir Isaac Newton had granted, that then it was not necessary I should sign any agreement with the referees, yet now he became very positive for articles.   He had said to some of his confidants, " that he would hamper me with articles." It had come to my ears, and, therefore, on his urging me, I drew up some for the undertaker to sign, as that he should print only 400 copies ; that he should have no interest in the original, &c.   But these were not to his purpose.   I would not court him.   To bring about his low designs, he makes articles himself, in which some things of mine were inserted, and in them he covenants the undertaker should print five sheets per week ; and for re-printing of faulty sheets; and that I should have £125 paid me, when ten sheets were printed off.   These were read to me once, and I was required to sign them immediately, else the work was at a stand : no time would be allowed to consider of them, or amend any thing I thought amiss in them.   I was then near £140 out of pocket: all my copy was ready for the press or soon would be.   If I refused, the work would be broken off immediately, and the fault would be thrown upon me.   For, Sir Isaac Newton lived in the neighbourhood of the Court.   I at six miles distance. He had his close friend, the Lord Halifax, to support him there, with the Prince's physician.   I had nothing but my sincerity and God's blessing to depend upon.   Trusting on these alone, I signed them; not doubting, but now the press would begin.   The articles are dated, November the 15th, in the 4th year of Queen Ann, or 1705.

But herein, I soon found myself deceived. This would not satisfy. I would not yet cry up Sir Isaac as others did. To bring me to that baseness, now he has got my book of *Night Notes*, he wants a copy of so much of the Catalogue as I had gone through with, to be trusted into his hands. He therefore demanded it; I answered, that it was not then perfected; that I believed it would contain a good number more than I had yet observed and rectified; that the stars already in it were about 1500, but, probably, I should make them 2500; that these were the result of all my labours, in which, having spent above £2000 of my own money, above my allowances, it would neither be prudent nor safe to trust a copy of them out of my own keeping. He answered, that I might then put them into his hands sealed up: whereby I understood, they were to be so kept by him, till I had finished the whole and was ready to print it. I considered also, that this half of my Catalogue would be of no advantage to him, and consented. I, therefore, delivered the copy of so much of the Catalogue as was finished into Mr. Hodgson's hands, with orders to seal it up in Sir Christopher Wren's presence, and deliver it to Sir Isaac Newton, when ten sheets were printed, and £125 (which would then be payable by the articles) should be paid me. This was March 8th, 1706, but this direction I waived afterwards, and it was put into his hands the week after, without receiving a farthing for the board or pay of my amanuensis or calculators. For *honest* Sir Isaac Newton, (to use his own words) would have " all things in his own power" to spoil or sink them; that he might force me to second his designs and applaud him, which no honest man would or could, and God be thanked, I lay under no necessity of doing.

This business being over, a week after meeting me in London, he told me he would now draw £800 of the Prince's money, but said nothing of paying me what I had disbursed. However, we must now put the work into the press, for, after such unreasonable concessions on my part, the pretences for further delay were all taken away, and he had no excuses for further delays.

April, 4th, being in London, I was told that all the errors which he, by mistake, thought he had found in my copy,

were quelled; and that the first sheets would go to the press this week.

April 19th. I waited upon him again: he told me gravely that the Prince having subscribed a great sum to the Emperor's loan, the money could not be received, but that he had taken up money for Mr. Churchill. This was to provoke me, but he failed of his design. Whatever I had hitherto expended I was content to adventure a little more. Mr. Churchill was put upon me, had never been at any expense; must have monies put into *his* hands beforehand, to buy paper and pay the printer, whereby he was sure to have him at his command. And, though it was covenanted that he should print but 400 copies, might take as many as he pleased; for I never heard nor found that he had given any bond or security for fair dealing, however, it was highly reasonable he should. But this was not all. The printer being to be paid by the undertaker, and not by me, was likely to be careless of his work, which I urged, but to no purpose.

It was May 16th ere the first sheet was printed off, and June 3d, ere we got a second; and the third on the 7th of June. So here was a whole month since the first was wrought off; and not two sheets (in the room of twenty that, by the articles ought to have been printed) in a month's time. I complained boldly of the dilatoriness, but in vain. All the answer I got was from Sir Isaac's own mouth, " that we must proceed slowly at first and make sure dispatch afterwards." This was one of the fruits of our having an undertaker, and leaving the printer to be paid by him, who neglected the *Historia Cœlestis* if they had but a sorry pamphlet to print.

We had got two alphabets (that is about forty-six sheets) out of the press by Christmas, 1706; and the whole, (5 E) or ninty-seven, before December 21, 1707; that is ninty-seven sheets in about eighty-nine weeks. In which time, had they printed five sheets per week, according to their articles, all the observations made with the mural arch from 1689 to 1706 might have been easily printed, as well as those made with the sextant.

In the meantime, Sir Isaac Newton sometimes stopt the press without assigning a reason for it, or any occasion

given by me; but upon my complaint at the first, and afterwards, without any solicitation of mine at all, let it go on again. I happened once to visit the press when he was there, and took the opportunity to show him how ill the compositor had placed the types of the figures, and how much awry to the lines to which they belonged (sheet K k k, p. 224). He put his head a little nearer to the paper, but not near enough to see the fault, (for he is very near sighted) and said, making a slighting motion with his hand, "Methinks they are well enough." This encouraged the printer in his carelessness; the sheet was printed off, and the fault not mended; and caused me to be more watchful over the printer. For now it was plain to me that the referee, as he called himself, was not displeased with the faults he committed; and the undertaker never concerned himself about them. He was sure of certain gains by his paper and press work, and some more probably than we were aware of.

The printing of the sextant observations being finished, I expected the press should have gone on after Christmas, with the volume of observations made with the mural arch, which were double the number of the other. But Sir Isaac Newton had put a full stop to the press, though he knew very well that the copy was ready, fairly transcribed, on 175 sheets. What excuse he made for it I know not; for none of his confidants would acquaint me.

In the meantime I had complained to one of the referees, who was often at Court, and waited frequently on the Prince of my ill usage; that care was taken of the undertaker and printer, but that none was taken to re-imburse me in the entertainment and pay of three calculators, and in transcribing the copy for the press, which came to more than £173, though I accounted nothing for my own and my servants' attendance on the press. He was ashamed of it; promised it should be redressed; and, I am apt to think, procured a meeting to be appointed on the 20th March following, which was notified to me, and I was then desired to bring with me what I had more by me ready for the press.

The press had now stood three months, by Sir Isaac Newton's only procurement. For to keep all things wholly in his own power he had brought in an undertaker who was useless to the business, and served only to spoil the work,

or worse; and a printer whom I believe he paid. I am
sure he never consulted me about the payment of either,
though there was sufficient cause; all the articles relat-
ing to them having been broken; but by this manage-
ment he had them wholly at his devotion. On the day
appointed (March 20, 1707-8) I took up with me to London
all the observations here made between September 1689
and December 1705, fairly copied in 175 sheets of large
papers. Six sheets were of the planets' places, calculated
from the observations made with the sextant, which ought
to have been printed next after the said observations, as
also a fair copy of the places of the stars in the ecliptical,
and as many of the southern constellations as I had then
rectified. The referees viewed them, and Sir Isaac, after
some time, withdrew, and calling Dr. Arbuthnot out to
him, produced the following, which the other referees, as
I remember, signed. He would not deliver it to me, but
gratiously permitted me to take a copy of it.

(Here follows the agreement, dated March 20, 1707-8).

There were present at this meeting, at the Castle Tavern
in Pater Noster row, Mr. Roberts, Sir Isaac Newton, *Sir
Christopher Wren,* Dr. Arbuthnot, Dr. Gregory, Mr.
Churchill, Mr Jamer Hodgson, and with myself, my ama-
nuensis Isaac Wolferman.

The conditions on which I was to deliver this second
volume were very hard and unjust: for the observations
contained (there) were most of them made with the new
mural arch, which I had built at my own cost, and lay me
in above £120 out of my own pocket. My own instruments
were all my own, too; and my assistants were paid and
maintained at my own charge. I had laid out, moreover,
above £173 in carrying on the works; of which I had given
a bill both to Sir Isaac Newton and several of the referees.
I considered that If I should not consent to this order, Sir
Isaac Newton would say that I had hindered the printing
of my own works myself; which would serve to justify a
report, spread by his partisans very industriously, that I
was averse to the publication of them. Whereas I had
always endeavoured to carry them on as industriously as I
could; and he had done all he could to hinder me, in order
to make me comply with them, and cry him up at the same

rate they did. Further, I saw that if I did not lay hold of this opportunity I could not hope to be re-imbursed any of the £173 I had spent in preparing the copy for the press, and performed my part of the agreement in the time agreed. But the £125 was not paid me till about two months after, and then I was still above £48 out of purse, for which I had nothing but three copies, one that I gave Mr. Sharp, and another in which I have corrected the faults of the press with my own hand, and a third not complete.

I was now in hopes that the press would begin again to work with the second volume; but when, after three or four months delay, I found that for all my instances there was not the least step made towards it. I complained of this behaviour of Sir Isaac Newton, both paying me short of what I had disbursed, and of his keeping the 175 sheets of copy for the second volume in his hands. This I believe was (as intended) carried to him. Whereupon, to throw all the fault upon me, eight months after he had stopt the press, he sent me the following order :—

" At a meeting of the gentlemen to whom his Royal Highness, the Prince, hath referred the care of printing Mr. Flamsteed's astronomical papers, it was agreed that the press should go on without further delay : and that, if Mr. Flamsteed do not take care that the press be well corrected, and go on with dispatch, another corrector be employed.

Whitehall, July 13, 1708."

To prevent the designed effect of this malicious order, or agreement, I wrote a letter to Sir Christopher Wren, (who, I believe, hated such practices), and sent it him in a few days after. I declined writing to Sir Isaac Newton, because he might suppress it; and I doubted not Sir Christopher would impart it both to him and the other referees. I took a copy of it to myself, to show my acquaintance, friends, and some gentlemen that had an opinion of Sir Isaac Newton before, and could not think he could be guilty of such collusion as this order, and my letter proved upon him. The letter was delivered, and imparted to Sir Isaac Newton as I desired it should be : yet I never received any answer to it. But the press was stopt, and no more talk of it this year; in the latter end of which the Prince of Denmark died, on October 28, 1708 ; in whom the Observatory lost one that would have been a great and noble patron, had he

not been prevented by one of his physicians, who was influenced and governed by Sir Isaac Newton.

Being now not disturbed with him any more at present, I set myself to carry on such observations as I wanted, and made good advances in it; adding many stars to some constellations that I had gone through before. But when I least expected it I was afresh disturbed by another piece of Sir Isaac Newton's ingenuity. After the Prince's death the old ministry was changed; a new one introduced: his patron was well with the chief of them; the Queen's physician was in his interest and the new Secretary of State's. It was not enough that Sir Isaac Newton had got my observations (made with the mural arc) into his hands by surprize, together with above half the catalogue, whatever my expenses had been, or pains in making it, so long as I would not leave myself and pains wholly at his disposal; and, therefore, he procures, by the means of the physician, minister, and Secretary St. John, an order constituting the President (Sir Isaac Newton) of the Royal Society, the Vice-President, and whom else they should think fit of the Society *visitors of the Observatory*. 'Tis dated December 12th, 1710, and was sent me by the office-messenger, on the 14th, with the Queen's letter intimating it.

The next morning after I received this, I waited on Mr. Secretary St. John, and told him that I was injured, and should be hindered by this new constitution of *Visitors;* that I wanted no new instruments; and that if I did, the Visitors were not skilful enough to contrive them; that for my repairs of the Observatory, the Office of Ordnance had hitherto taken care of them, and would now as soon as the weather should be fit; that the instruments and clocks in the house were all my own, and that I had hitherto repaired them all at my own charge; that I had expended above £2000 more than my appointments, in instruments and assistants; and that it would be very unjust to go about to deprive me both of the honour and benefit of my own labour and expenses, and confer them on those, who had done nothing but obstruct and hinder me in all they could, and wanted to boast of their merit in preserving my labours, because they had nothing of their own worth the public view. Mr. Secretary St. John seemed not to regard

what I said, but answered me haughtily, " The Queen would be obeyed." The Lord Rochester, the Queen's uncle, living near the Secretary's office, I also waited upon him, and shewed him what tricks and disingenuous usage were put upon me by Sir Isaac Newton; and though I found no immediate advantage by it, yet I am apt to believe, it was of use to me afterwards.

Sir Isaac Newton valued himself very much upon the suggestion, that it " would contribute very much to the improvement of astronomy and navigation, if there were constant visitors appointed of the Observatory, &c.;" and, one of the principal of the council of the Royal Society could not forbear to speak of it to me in public company.. Whereas the contrary is evident, from what had happened to the noble Tycho, who had no Visitors of his Observatory appointed over him, during the reign of his patron, king Frederick II. When some persons were appointed in the following reign of king Christian, they were such, as were very unfit for that purpose, much less skilful than himself, and made use of purposely to asperse him, only to make him uneasy and withdraw, that the courtiers might get his appointment, (which were 2000 dollars a year allowed him from the Treasury, a fee in Norway, worth 1000 dollars a year more, and the prebend of Roschild, of 1000 more) into the King's hand again, which they did; and soon by him were conferred on the Templars. My appointments, though very small in comparison of his, were also designed by Sir Isaac Newton for other persons that would be dependant on him; and this expedient of Visitors was to perform strange things. But the good Providence of God so ordered it, that I received but little damage by it: and he got little but shame and disgrace for his ingratitude in disturbing me in my business, which he was bound by his oath to assist me in, as President of the Royal Society, and as chief (as he had made himself) of the Prince's referees, or indeed, *the all of them.*

But, now that he got another pretence of authority, to make me sensible of it, a report was spread, that a letter was coming to me from the Royal Society. This was in the beginning of December, 1710, and was occasioned, I believe, by their knowing of Mr. St. John's letter that was

brought to my hands on the 14th. I heard nothing of any letter from them : if they then designed any, I believe, on better thoughts it was laid by. But in March following, I was surprised when I was privately told, that my Catalogue (which I was then working upon to complete it as far as I then could) was in the press: but more with a letter of Dr. Arbuthnot, dated the 14th March, 1710-11, wherein he very confidently required of me the copy of the star's places of six constellations, viz. Draco, Ursa Major, Ursa Minor, Cepheus, Cassiopea, Hercules, that had not been delivered into Sir Isaac Newton's hands, when he got the rest into his possession by tricks and pretences. This, I believe, was one of the boldest things that ever was attempted. None that had less dexterity, and boldness, and art, than the Doctor, would have had the confidence to have mentioned such a demand. I had made my instruments, and maintained my assistants at my own charge without complaint of it ; so long as I could be quiet and undisturbed by the small people that cried him up. I had put a copy of that part of my Catalogue which was in order into his hands, to be preserved in case of my mortality, and to prevent it from being lost by accidents, and to let him see that I could go on with it as soon as I had determined the right ascensions and distances from the pole, of other stars in other constellations. I gave him also copies of them, never designing or intending that he or any but myself should publish them. Nor, indeed, could any one else, for more observations were still wanting to complete it, and I was adding to it, adding or correcting something in it every day. Some letters passed betwixt me and the Dr. Arbuthnot, wherein he still urged me to give them the copy of the constellations, only wanting, as he thought, to complete my Catalogue: which I always answered civilly, with such just excuses as are above suggested ; desiring still that I might see him, either at the Observatory or in London, where at last he met me, on March 29th, and when I inquired of him, whether the Catalogue was printed or no, he assured me " not a sheet of it was printed." I answered him not, for I was sure it was ; because he then offered (in the hearing of Mr. Hodgson, and another gentleman I had taken with me to be a witness of our conversation and discourse),

to pay me £10 for every press fault I should find in it; and, within four days after, a friend sent me the constellations of Aries and Taurus, fairly printed; and, in a day or two after, that of Virgo. So that I was now convinced that the press was at work, and that the Doctor had told me *what he knew was not true.* I learnt, at the same time, (what had been intimated to me before) that Mr. Halley took care of the press, and pretended that he had found many faults in my catalogue; showed some sheets of it publicly in Child's Coffee-house, at St. Paul's, and boasted what pains he had been at in correcting them.

I had told Dr. Arbuthnot, in one of my letters, (April 18th, 1711), that one of Dr. Halley's best friends, and the wisest of them, had said of him, " that the only way to have my business spoiled effectually was to trust it to his management." Now, the truth of this expression was proved: for I found not only the names of the stars in my catalogue altered, but the numbers also in many places changed, and others put in their room that were sometimes fifteen minutes false; and, therefore, it was very effectually spoiled. And, by boasting of these corrections, as he called them, he would insinuate to the world that they were more obliged to him for his pains in correcting than they were to me for above thirty years spent in composing it; the cost of making instruments and hiring assistants at my own charge. For, by altering the names (to make them agree with his own faulty hemisphere) he had made himself in some sort (but a very bad one) a proprietor in that catalogue he printed without my name to it, or ever consulting me about it; which I would never consent to, as they well know by my letter to Sir Christopher Wren, which had been imparted to Sir Isaac Newton; and Halley was not ignorant of it.

On June 23, 1711, he delivered to my niece, Mrs. Hodgson, a fair copy of all the sheets of the catalogue, but without any preface to it. When I examined it I found more faults in it, and greater than I imagined the impudent editor either could or durst have committed. He had taken no care to put those into their proper order which I had left digested to his hands; because I had not yet got occasion to complete the constellations to which they belonged, particularly

the stars of Hevelius's new constellations with Hercules, Cassiopeia, and the two Bears. In some places he had altered the stars' right ascension and distance from the pole, and made them false which were true before.; and in the constellation Draco there were not above six or eight stars that he had not corrupted. Besides, I had added above thirty stars to the constellation; as many to Hercules; and so many on others, that the total number of them, in my own catalogue, would be near 400 more than there were in those papers I had intrusted Sir Isaac Newton with, to preserve in case of accidents, and which he had betrayed into Halley's hands, when he had been told of his qualifications before. Therefore, finding no other remedy, I resolved to reprint it at my own charge. I procured a couple of expert calculators, (Mr. Ab. Ryley and Mr. Crosthwait), corrected his faults and blunders, got the places of the stars lately observed calculated by both of them for greater certainty, made a new copy, in which the ancient names were restored, Hevelius's constellations inserted amongst the rest in their proper places, and in the order I first designed. But paper was exceeding scarce and dear, because of the war with France not yet over; which delayed the printing my intended edition corrected and enlarged. In the meantime, Sir Isaac Newton summons me to meet him at the house of the Royal Society, in Crane Court, October 26, 1711; where I found him with Dr. Sloane, Dr. Mead, and one more that I knew not, but I believe was his or their clerk at the time. He called these three, with himself, a committee, and told me they had sent for me to know what repairs I wanted, or instruments? I told them that the Office of Ordnance took care of my repairs; that it was now too late in the year to set about them, but that as soon as the spring came I should have that done which was necessary; and as for my instruments, they were all my own, either given to me by Sir Jonas Moore, or made by myself at my own charge, and always repaired at my own expense: And further, that I would not suffer any one to concern themselves about repairing of my own instruments, in which, and necessary assistance, I had spent £2000. The impetuous gentleman hereupon said, " As good have no observatory as no instruments." And soon conceiving that I apprehended his

design, and obviated it by my answers, broke out into a passion, and used me as I was never used before in my life. I gave no answers, but only desired him to be calmer, and moderate his passion; thanked him for the many *honourable* names he gave me, and told him God had blessed my endeavours hitherto: that his wisdom was beyond the wisdom of men, and that I committed myself to him. Dr. Mead seconded him, unprovoked, in his ill language; but Dr. Sloane held his face. I thanked him for his civility; permitted him to help me down stairs; and, at the door, met Halley, who had not been far off all the time; and, I believe, heard Sir Isaac Newton shew his best g****† It would be too long to give an account of it all : there is a longer in my old book of letters, A, page 104-105, where those who come after me will find it. I pray God forgive him. I do. I do not remember that I ever saw the observations of mine (printed at the same press with my corrupted catalogue) till three years after ; when there were 300 copies of the printed edition of the Observations given me (as they were designed) by King George. The whole was intended for me, by the Prince George of Denmark, but I was forced to be content with this part of them, and took them with thanks. I found them as much corrupted as the Catalogue, but if God spares me life I hope to present the world with a perfect edition of them, the editor having transcribed only the Observations of the Planets, and made a sorry and fallacious excuse for his omitting the Observations of all the fixed stars that were not employed for finding of the planets' places.

On the 18th of June, 1712, the impudent editor, with wife, son, and daughters, attending him, and a neighbouring clergyman in his company, came hither. I said little to him. He offered to burn his Catalogue (so he called his corrupted and spoiled copy of mine, of which I had now a correct and enlarged edition in the press, and the second sheet printing off) if I would print mine. I am apt to think he knew it was so, and ·was endeavouring to prevent it. But to render his design ineffectual I said little to him of it; so he went away not much wiser than he came.

Saturday, August 1st, 1712. Sir Isaac Newton came

† The remainder of the word is illegible.

himself, accompanied with Dr. Thorp, Mr. Machin, Mr. Rowley, and Mr. Hodgson, who had given me notice of their coming before-hand. I had provided Mr. E. Clark, and Mr. Ryley to attend our conversation, and accompany them to view (the) house and my instruments, being a little lame myself with the gout. They had a view of what they pleased, except my library. I gave them a glass of wine. Sir Isaac promised to return me a Greek Ptolemy he had borrowed of me, and 4 vols. in quarto, of the First Night Notes, which he had kept in his hands now about six years, to no other purpose but to show his authority and *good* nature; and returned (them) not till more than four years after, when I had commenced a suit against him for them.

This business being over, and Sir Isaac Newton finding that his visitation had not the effect he promised to himself, he took care to let me know by the Secretary's letter as soon as the year 1711 was expired, that the Royal Society expected the copy of the Observations of that year. I returned an answer to him that they should have them in the time prescribed by the order; and, accordingly, caused my amanuensis, Jos. Crosthwait, to transcribe and leave them at their house in Crane Court, some days before Midsummer 1712. I expected that they should have sent me a receipt for them, but *civil* and *just* Sir Isaac Newton esteemed it too great a favour for me. I did the same for the year following, on a second letter from the Secretary of the Royal Society. And in the year 1713-14, I found them both printed, abridged, and so spoiled by the editor of my Catalogue, that I would no longer own them for mine. The most material observations were omitted, and the rest so managed that it seemed to me he had designed to spoil them, out of spite. He had inserted some that were imperfect; and given the right ascensions and distances of the planets from the pole deduced from the observations; but not their longitudes and latitudes. This was too much drudgery for his acuteness, who was used to procure what he published as his own at easier rates.

After the same manner he got my observations of the year 1713 into his hands; abridged, spoiled, and printed them in his Transactions for the year 1715, No. 344. But the Queen deceasing before they could lay any claim to the

next year's, and their authority ceasing, I declined answering their further demand; for their authority ceased. Yet their confidence did not: and the editor, (Dr. Halley), who now was one of their secretaries, sent me a bold letter to demand them, as if he had never done me any injury; which I laid by me, and kept that year's from being spoiled. How unfaithful he was in his copy I hope the skilful may see ere long: for my amanuensis, J. Crosthwait, is now copying the volume of observations that Sir Isaac Newton got by surprize into his hands, and has nearly finished it. And I hope I may live, through the blessing of God, to see it published, with the observations of twelve following years: but if His good providence shall not continue my life so long, I trust my executors will do it according to the direction of my will.

The last sheet of my corrected and enlarged Catalogue was printed off, December 5th, 1712; after which I designed to have had the press to proceed with the observations from which it was derived, made with the mural arch. But whatever instances I made to Sir Isaac Newton to have the copy I had trusted into his hands, to be printed, I could not prevail with him to return it. So I set myself to continue my observations, at such times as were fit, further; and to calculate the planets' places from such observations as I had made with it; and to correct the places of the planets' motions. In which, I bless God for it, though I had not the success I expected, yet I had such as gave me light, and will be of use to those who come after me; and may serve to perfect our knowledge of the Heavens, wherein the height of wisdom is shewn of our Creator; if after me there shall be any found that will prosecute these studies with the same sedulity, patience, and sincere love of truth that I have now for above these five-and-fifty years.

August 1st, 1714. King George succeeded to the crown of Great Britain. Soon after a noble peer died, who, during his life, had supported Sir Isaac Newton (the Earl of Halifax). The officers at Court were changed. The new Lord Chamberlain knew me well: and one that was frequently employed by him wrote to me, that through his means I might get the printed copy of my observations that had been designed for me by the Prince George of Denmark, into my

hands with little trouble: the Lord Chamberlain having, by his office, the care of his library. I thanked God for so good an opportunity. My friend, with the Duke of Bolton, did his best: but, after all, we find the Lords of the Treasury had the power of disposing of them. Mr. Walpole was first commissioner. Mr. Methven, unasked, became my friend: Mr. Newport, (now Lord Torrington) I (had) been acquainted with long since. I caused a memorial and petition, wherein my case was truly represented to them, to be drawn up and delivered. Whereupon 300 copies were ordered to be delivered to me by the undertaker, Mr. Churchill, who, by his articles, was bound to print but 400. I brought them down to Greenwich; and finding both Halley's corrupted edition of my Catalogue and abridgment of my Observations, no less spoiled by him, I separated them from my Observations; and some few days after I made *a Sacrifice of them to Heavenly Truth;* as I should do of all the rest of my editor's pains of the like nature, if the Author of Truth should hereafter put them into my power; that none of them but what he has given away, and sent into foreign countries, may remain to show the ingratitude of two of my countrymen who had been obliged by me more, on particular occasions, than any other mathematical acquaintance; and had used me worse than even the noble Tycho was used in Denmark. And I should have felt the effects of their malice and envy more, had not the good providence of Almighty God prevented them.

Whilst I was soliciting this affair in the Exchequer, Sir Isaac Newton was passing his accounts there concerning the disbursement of the Prince's monies. He would never own to me what the Prince allowed for the charge of printing, lest he should quit any part of that power he pretended (and he would gladly have me to have thought him) to have had. I have heard that the Prince designed £1200 for the printing. Dr. Keel told me £2500; which I am apt to believe is true; the other £1300 being not less than the engraving of the maps of the constellations and other figures will cost. But here I learnt that Sir Isaac Newton's accounts specified £150 given to Dr. Halley for the pains he had been at for correcting (as he calls it) and publishing my Catalogue. And to one of his servants for assisting him in

calculating the places of the stars, £30. So that Isaac
Newton had wasted £180 in spoiling of it. Besides, he told
me that he had given £20 more to the poor Frenchman
that drew and engraved the flattering figures for the frontis-
pieces or capitals; upon his complaint that the first agree-
ment was too hard a bargain. So that there was £200 of
the Prince's money thrown away, only to show his liberality
unnecessarily, which evidently proves his ignorance of the
business. For the Catalogue was very correct before his
editor corrected it: and the designer or engraver of the
frontispiece, or capitals, knew, no doubt, how to make a
bargain for his pains. The editor and his calculator were
both indigent: (and he) found this way of relieving them,
without any expense to himself; and making them open
their mouths wide in crying him (up for) his liberality as
they had done before for his skill in what he is (no master)
of. Whilst my amanuensis, J. Crosthwait, was at more
pains in (correcting) their faults and calculating the places
of 400 stars (more) than were in my first copy, without any
allowance (more) than the yearly wages I gave him.

Having thus got my own printed observations and Cata-
logue into my own hands, I caused the observations of Mr.
Gascoigne and Mr. Crabtree, made in Yorkshire and Lan-
cashire, in the years 1638-1642, together with my own
made at Derby, between the years 1669 and 1675, which I
had mentioned in my estimate (as these were to compose a
part of my first volume of observations), to be printed in
Latin: together with a small table for turning the parts
measured by the micrometer (either in the longer or the
lesser tube) into minutes and seconds of a degree. I also
sent to Sir Isaac Newton, to return me the 175 sheets trusted
into his hands, March 20th, 1708-9, to be printed.

But, finding he delayed to restore, or even flatly denied
to do it, I set my amanuensis to copy them, in order to
have them printed; that they might be published together
with the Catalogue in their proper order, which I had first
proposed in my said estimate, and which I endeavoured
always to preserve; whilst Sir Isaac Newton as pertina-
ciously contended to obstruct and break, that he might
thereby force me to some mean submission, to procure his
consent. Though the work was nothing of it his, he had

concerned himself with the Prince George of Denmark without my consent, in the edition; and was so bold, as by his creatures to intimate to me what he wanted; but the cunning failed him; the sheets will be copied in a short time; and I hope, if God spares me health one year more, I may see them all printed and fit to be published.

Having thus given the history of my observations of the fixed stars, and shewn both what the true obliquity of the ecliptic, or the inclination of the earth's axis is, as the assertors of their motion would rather call it, and how it came to pass, that I have met with so many obstructions and hindrances in the preparing the Catalogue of them for the press and publishing of it; having also shewn how I determined the inequality of the earth's motion, and the true places of some of the principal (stars in the Catalogue) and from them all the rest inserted in it, I shall next give an account of such variations as may be caused in their right ascensions and distances from the visible pole, by the *Parallax of the Earth's orbit.*

From my first year's observations of the pole-star's meridional distance from the vertex, I supposed that the parallax was sensible. Some observations I had taken with the sextant, of the intermutual distances of bright fixed stars had caused me to suspect it before, for I found that I had them at some times of the year, some little bigger than others. But the sextant being an unfixed instrument, that required two persons to make use of it, and the air being changeable, and different at different times of the year, and consequently, the distances being more or less contracted by refractions, according to the greater or less density of the air, or greater or less inclination of the planes passing through the two observed stars, to the vertical circles falling upon them, it was very difficult to make any good conclusion from them. Continuing, therefore, my observations of the pole-star yearly, I found always a small, but sensible difference, betwixt those I took in September, and the following months of each year, which argued a sensible parallax in the star.

[An account was given of these observations, in a letter from Flamsteed to Professor Caswell, published December 22nd, 1698. By the observed distances of the pole-star

from the pole, it was found, that the greatest exceeds the least by 40″ or 45″, and, therefore, the greatest parallax of the orb at this star is more; and, probably, 50″ or very near a whole minute.

When Flamsteed obtained the 300 copies of his printed work, edited or mangled by Halley : he destroyed only the Catalogue and the spurious part of the work, which professed to be his observations made with the mural arc. That portion containing his observations with the sextant were separated from the rest, and now forms with the observations of Crabtree, &c., the first volume of the *Historia Cœlestis.* So that of all the three volumes of the *Historia*, only 97 sheets of the first volume were printed at the public expense, all the rest having been edited at the risk and private cost of Flamsteed himself. He died, however, before the second volume was completed. This occurrence is related in a letter from Mr. Crosthwait to Mr. Sharp, dated " Observatory, January 2nd, 1719-20, He was taken ill on Sunday last, about a quarter-past 12 at night, and continued to vomit up every thing he took, till Thursday night, when about 38 minutes past 9, it pleased God to take him. I shall always lament the loss the public will have of so valuable a man." Thus lived, and thus died this " great and good man," as he is designated by his intimate friend Crosthwait. May the jealousy with which he was persecuted during his life, receive its just meed of reprobation from posterity, and may the calumnies which have hitherto thrown a gloom over his great talents and worth, be for ever sunk in oblivion, and general respect and admiration substituted in their room.]

ARTICLE II.

An account of the process of making Spirits, in Great Britain and Ireland.
(*Continued from* vol. ii. p. 465.)

6. *Wash Charger.*—The wash having been fermented as above described, is conveyed to the wash-charger, which is simply a measuring vessel; its cover and communications with the still and with the wash-backs are under the

Revenue officers' control; being kept locked at all times, except when wash is being conveyed thereto from the backs; but so soon as it has been filled, the officer locks the communications between it and the backs, and opens the communication between it and the wash-still, to allow the wash to be run into it; so that no more wash can be run into the charger till the officer has again opened the first mentioned communication; and thus the process goes on, charge after charge.

The *Stills.*—Common stills are so well known that a description of them here would be superfluous; any boiler having a head and worm, or refrigator, attached to it, will answer the purpose, its object being simply to separate the spirits, or alcohol, from the wash in which it is contained; this is obtained by applying heat to the wash, until its temperature is sufficiently raised to convert the spirits into vapour; this vapour escapes through the head of the still, into a spiral tube, technically called the worm; which being contained in a tub or tank of cold water, condenses the vapour into a weak impure spirit, called low wine. These are run from the worm, and into the low wine receivers, in which they are taken account of by the Revenue officer. They are immediately afterwards pumped up into another vessel called the low wines and feints-charger, and from thence run into the low wines still, which is furnished with a head and worm like the wash-still. In this still the low wines are re-distilled, and produce a quantity of pure spirit, which issues from its worm end, and is conveyed into the spirit-receiver as a finished article, fit for consumption. But a large portion of this second distillation is still too coarse, and too much contaminated with essential oils, for commercial purposes, until it has undergone further rectification. These coarse spirits, whether the produce of two or more distillations, are called faints; they are carefully separated from the purer portion, and conveyed into a vessel, called the "faints receiver," from which they are again removed into the still, and re-distilled repeatedly, until the produce becomes pure, which finishes the process.

Up to the termination of the last century, the common still and worm, such as we have just described them, were uni-

versally used in the distilleries, not merely in these islands, but also on the continent of Europe, and in the West Indies; and the process consisted of repeated distillations, in order to bring the alcohol to a sufficient degree of purity. It is true that philosophical men, long before that period, saw that the apparatus admitted of improvement, and that the process, as ordinarily carried on, wasted much time and fuel. There are suggestions, in the older chemical books, of plans for obtaining alcohol of a high strength by a single distillation; but it was not until the year 1801 that the idea was carried into effect in the large way, by Edward Adam, a distiller at Montpellier, in the south of France, who, in that year, invented a distilling apparatus which was immediately adopted in a great number of the French distilleries. This invention was followed, shortly afterwards, by another, brought forward by Isaac Berard, also a distiller in the south of France. Berard's apparatus was devised for the same purpose as that of Adam, namely, the production of strong spirits by a single distillation; and, being less complicated, and less expensive in its construction, it was by many preferred, although it did not, by any means, economise fuel to the same extent as the apparatus of Adam.

The introduction of these two improvements, in a manufacture of so much commercial importance to France, immediately drew the attention of many ingenious individuals; and, during the first twenty-five years of the present century, from 1800 to 1825, an immense number of inventions were brought forward for improved distilling apparatus. Dr. Solimani, Curaudau, Cardonel, Chaptal, Clement, and other eminent men, lent themselves to the task, and an apparatus was at length perfected, and very generally adopted, which combined the advantages of both Adam's and Berard's inventions, and performed the process of extracting the purest alcohol, in a single distillation, with great economy of fuel.

At the same time that these modifications of Adam's and Berard's principles were carrying into effect, a distiller at Bourdeau, named Baglioni, conceived the idea of making an apparatus which should work *continuously*, that is to say, should be constantly receiving a stream of wine, or wash, at one end, which would constantly flow off, boiling hot, and exhausted of its alcohol, at the other; the alcohol, at

the same time, flowing continuously into a proper receiver. M. Baglioni's first efforts were not quite successful; his machine did not effectually exhaust the wine, and a portion of alcohol was, consequently, lost, by those who adopted his apparatus.

The idea, however, was followed up by M. Cellier Blumenthal, who contrived a more perfect machine, and finally, an apparatus was constructed by Dr. Charles Derosne, which completely solved the problem, and which has, perhaps, carried the distilling apparatus to the *ne plus ultra* of perfection, so far as relates to the distillation of clear transparent liquors, such as wines usually are.*

It would take up too much space to give any detailed description of these inventions here, and would, besides, be foreign to the intention of this paper, which is to describe the practice of Great Britain and Ireland. But the reader who feels curiosity on the subject will be amply gratified by consulting Le Normand's Treatise on Distillation, published at Paris in 1824: That of Fried. Hermbstaedt, at Berlin, in 1823: Dubrunfaut's work, published at Paris, 1824; and Peclet's Treatise on Heat, Paris, 1828.†

Several causes prevented the introduction of these improvements into the distilleries of Great Britain and Ireland.

1. The Excise regulations chalked out a particular course in which the distiller should carry on his processes, to which the new machines could not be adapted; and the heads of the department felt a natural dislike to innovations.

2. The spirits being made from corn, large quantities of hot water were required for mashing this corn, and the vapours from the common still, during their condensation, heated water for that purpose; the heat, therefore, was not lost, as in the French wine distilleries, where all the hot water runs to waste, there being no use for it. The saving of fuel, therefore, arising from the use of the improved ap-

* The fact is disputed as to the original proposer of continuous distillation. Some assert it was M. Blumenthal, not Baglioni: others trace the suggestion farther back than the apparatus' of either; but it is an undoubted fact that Dr. Derosne was the first that brought the idea into complete practical effect.

† We are not aware of any work, in English, on this subject, to which our readers could be profitably referred. We believe a part of Dubrunfaut's treatise has been translated by a Mr. Sheridan, but we have not seen the publication.

paratus, was by no means so important in a corn as in a
wine distillery.

3. Corn wash is not so well adapted to the new process as
wine; it always contains a great quantity of vegetable
matter, mechanically suspended in it while in motion, but
which, when the wash is in a quiescent state, rapidly falls
in a thick sediment on the condensing surfaces of the appa-
ratus, and destroys their power. Dr. Derosne's apparatus,
the most perfect of all those heretofore invented in France,
is not at all fit for the distillation of thick corn wash. A
modification of it is used in several of the corn distilleries
of Holland and Belgium, but its advantages are not com-
parable to those it affords in the distillation of wine.

It is probable that these causes would have prevented
the introduction of the new system of distillation to these
islands up to this day, were it not for a great change which
took place in the Excise law in 1823. Previously to that
date, the processes of brewing and distilling were carried
on *simultaneously ;* but, for the more effectual collection of
the duty, a law was then passed, requiring that the distiller
should suspend altogether the mashing of corn, or making
of new wort, so soon as he began to distil the wash then
in his possession ; and forbidding him to resume the making
or brewing any more wort or wash, until all he had on hand
when he commenced distilling had been worked up. His
manufacturing processes were thus divided into two distinct
periods, as to time, technically called, by the Excise officer,
the Brewing period, and the Distilling period. In conse-
quence of this division, the water heated by the vapours of
his wash and low wine stills, became useless to him ; and
it was not until then that the distillers of these countries
shewed any disposition to adopt the improvements of their
continental neighbours.

The first apparatus submitted to them was introduced by
a M. St. Marc, who took out a patent for it about 1827.
It is, however, the invention of M. Alegre, a gentleman
whose talents are highly spoken of in some of the works on
distillation to which we have referred our readers. It is a
very ingenious machine, and well adapted to the distillation
of *clear* wine, but, like all the others invented in France, is ill
fitted for the distillation of corn wash. Some of St. Marc's

stills have been put up by rectifiers, and, we believe, have given satisfaction; but we are not aware that there is one in Great Britain for the distillation of *wash;* and, we have heard that a few which were erected in Ireland for that purpose have been laid aside. A very full description of this apparatus will be found in the "London Encyclopedia" *voce* Distillation.

Shortly after the date of St. Marc's patent, Mr. Robert Stein took out two patents for distilling apparatus, the second being for improvements on the first: one of the principles of this apparatus is to keep the wash constantly in motion, in the form of a shower, or spray, by means of force pumps, by which it is injected into various chambers, wherein it comes into contact with vapour in a very subdivided state, and is thereby stripped of its alcohol. This requires the use of a great number of force pumps, which give to the apparatus a very complicated appearance, and require considerable mechanical power. There is great ingenuity and mechanical skill displayed in the construction of this apparatus, but it has not received much patronage; there is one of them in use at the patentee's own distillery, at Kirkliston, and another at Cameron Bridge, both in Scotland.

The third, and last, distilling apparatus we shall have occasion to mention, is that of Mr. Æneas Coffey, of Dublin, for which he took out a patent in 1832. We have seen two of those machines in Scotland, one at the Inverkeithing distillery, and the other at Bonnington, near Leith; and, in our judgment, this invention bids fair to supersede all other modes of distilling corn wash. We have obtained a drawing of the Inverkeithing apparatus, which will enable us to convey to our readers a tolerably clear idea of its construction, and, indeed, a tolerably clear idea of the general principles on which all the modern improvements in distilling apparatus are founded, for they are all embodied and carried into effect in Mr. Coffey's apparatus.

To render our description the more easily understood, we shall premise the following facts, to be kept in the reader's recollection:

1. Water boils at about 212°, and alcohol, the purest hitherto obtained, at about 171° or 172° of Fahrenheit.

2. Mixtures of alcohol and water boil at temperatures

Fig. 2

Q

L

M

Engraved by W. Kelham, London.
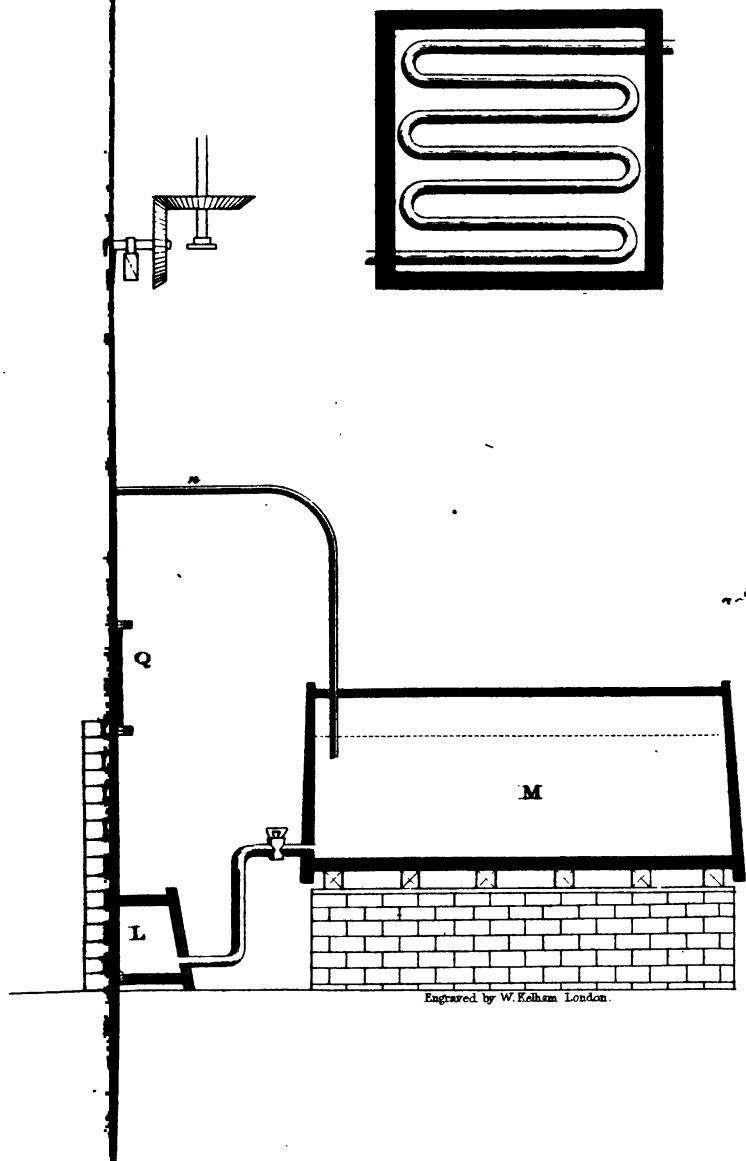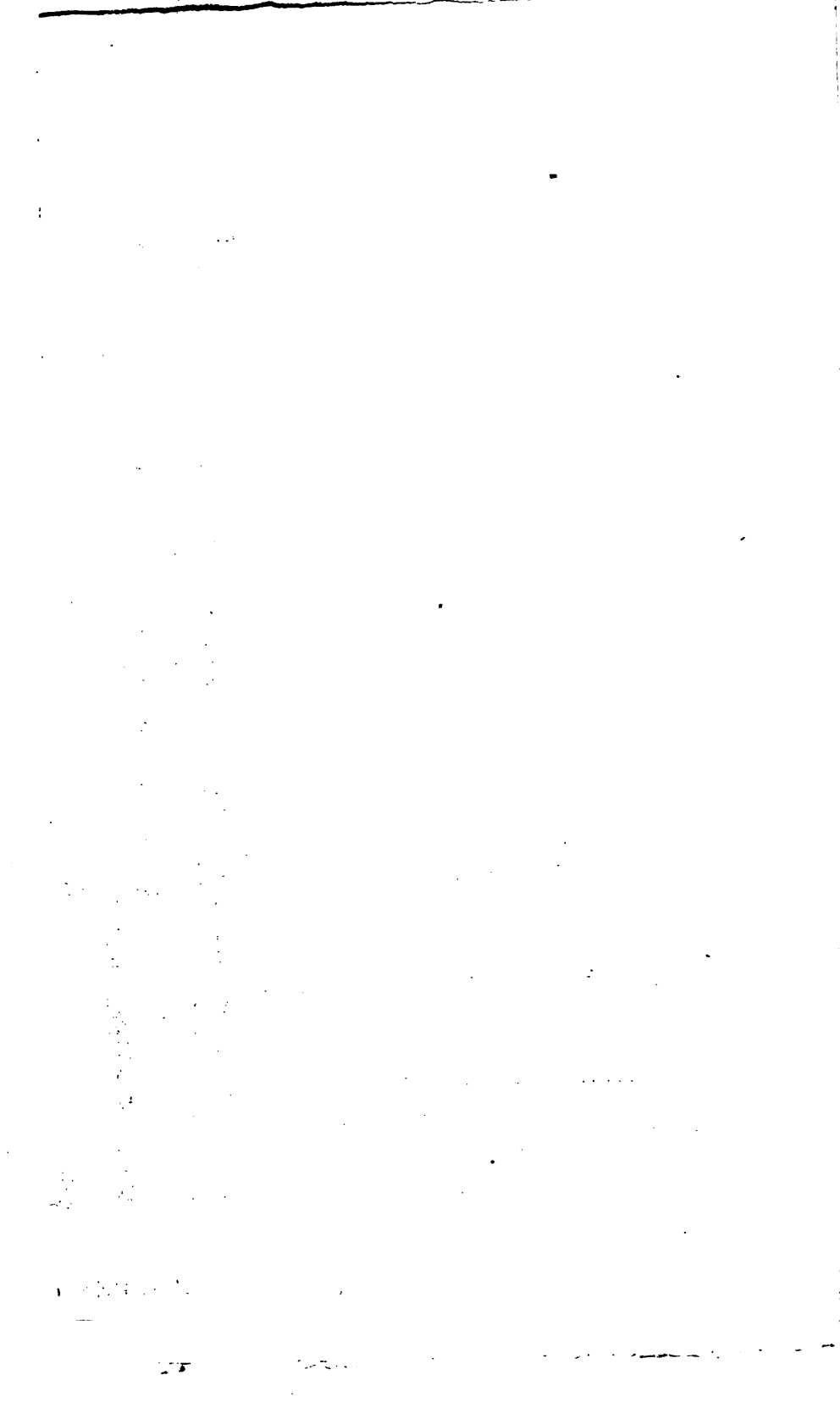
intermediate between 212° and 172°; the boiling point being higher or lower, as the proportion of water in the mixture is greater or smaller.

3. When the steam of water is thrown into, or blown through a liquid composed of water and alcohol, this steam will be condensed until it has given out heat enough to raise the mixture to its own boiling point, after which a portion of alcohol is volatilized and thrown off in vapour, by the further application of steam to the mixture.

4. When a mixture of steam and alcoholic vapour is blown through a liquid composed of water and alcohol, similar effects are produced; for, after the liquid mixture has arrived at its boiling point, the vapour blown through it lets go some of its watery part, or steam, and an equivalent quantity of alcohol is volatilized, and the mixed vapour, after passing through the liquid, carries off a larger proportion of alcohol than it brought with it.

5. When a mixture of steam and alcoholic vapour passes into a condenser, or worm, the vapours first condensed will contain more than a mean proportion of steam, or watery vapour; and, if the size of the condenser, or worm, be not sufficient, or the temperature of the bath in which it is immersed be too high to condense all the vapour, that portion of it which escapes uncondensed will contain more alcohol than the portion condensed.

Requesting our readers to keep these premises in mind, we shall now proceed to the description of Mr. Coffey's apparatus, in which, as we have already said, they are all brought into action.

The body of the apparatus consists of an oblong vessel, BB', and two columns erected thereon, C, D, E, F, and G, H, I, K,.

The first of these columns is called the *analyzer*, the second the *rectifier.*

The whole is made of wood, lined with copper, and the wood being five or six inches thick, little or no heat is lost by radiation.

The oblong vessel has a copper plate or diaphragm *c d* across the middle of it, which divides it into two chambers B B'. This diaphragm is perforated by a great number of small holes, for the passage of the vapour upwards during

the process, and it is also furnished with several valves which open upwards as shewn at *e, e, e, e,* whenever the vapour is in such quantity as not to find a free passage through the perforations.

A pipe V, V, descends from this diaphragm nearly to the bottom of the lower chamber B, into a pan forming a steam trap, and there is a valve on the top of this pipe which can be opened or shut at pleasure, by means of a rod *t,* passing through a stuffing box on the top of the vessel. Glass tubes at *x, x,* shew at all times the level of the liquor in the chambers BB'.

The column C, D, E, F, which is called the *analyzer,* consists of twelve chambers *f f f f f* formed by the interposition of eleven copper diaphragms *gh gh,* &c., similar to the large diaphragm *c, d,* that is to say, these eleven diaphragms are perforated with very numerous holes, and furnished with valves opening upwards. To each of them is also attached a dropping pipe *p p p,* &c., by which the liquor is allowed to flow from plate to plate; the upper end of each of those pipes projects an inch or two above the plate in which it is inserted, so as to retain at all times during the distillation, a stratum of wash of that depth on each diaphragm; the lower end of each pipe dips a little way into a shallow pan lying on the diaphragm underneath, forming thus a steam trap by which the escape of vapour through the pipe is prevented. The pipes are inserted at alternate ends of the diaphragm as shewn in the figure.

The column G, H, I, K is divided, in a similar manner to that just described, into chambers by interposed copper plates or diaphragms. There are 15 chambers in this column, the lowermost ten *k k k* &c., constitute the *rectifier,* and its diaphragms are perforated and furnished with valves and dropping pipes, precisely similar to those of the *analyzer.*

The uppermost five of these frames form the *finished spirit condenser,* and are separated from the other ten by a copper sheet, or diaphragm, without small perforations, but having a large opening at W for the passage of the spirituous vapour, and a dropping pipe at S.- There is a neck about the opening W, rising an inch or so above the surface of the diaphragm, which prevents the return of any finished spirit by that opening.

Under the dropping pipe S, is a pan much deeper than those of the other dropping pipes, and from this pan a branch pipe *y* passes out of the apparatus, and carries the condensed, but still very hot spirits, to a worm, or other refrigerator, wherein they are cooled.

The chambers *k' k' k' k'* of this finished spirit condenser, are formed of plain unperforated diaphragms of copper, with alternate openings at the ends, large enough both for the passage of the vapour upwards, and of the condensed spirit downwards; the use of these diaphragms being merely to cause the vapour to pass along the pipes *m m* in a zig zag direction, and to be thus more perfectly exposed to their condensing surface.

In every chamber, both of the finished spirit condenser and of the rectifier, there is a set of zig zag pipes, placed as shewn in the plan, figure 2, each set of these pipes is connected with the others by the bends *l l l l*, and they thus form one continued pipe *m m*, leading from the wash-pump Q to the bottom of the rectifier, whence it finally passes out at N, and rising up, enters the top chamber of the analyzer, where it discharges itself at *n'*.

M is the wash charger, L a smaller wash vessel connected with it and with the wash pump, this vessel is called the wash reservoir, and is not strictly speaking, a necessary part of the apparatus; its use is to retain a sufficient reserve of wash, to prevent the apparatus being idle during the delay, which the Excise regulations render unavoidable, between the emptying of the wash charger, and the re-filling it from a new back.

The pump Q is worked continuously during the distillation, so as to supply the apparatus with a regular stream of wash. It is so constructed, as to be capable of furnishing somewhat more than is necessary, and there is a pipe *n* with a cock on it, by which part of what is pumped up may be allowed to run back, and the supply sent into the apparatus regulated.

A is a steam boiler having nothing peculiar in its construction, the steam from it is conveyed into the bottom of the spent wash receiver by the pipe *b*, which, after entering the receiver, branches into a number of smaller pipes perforated with holes, by which the steam is dispersed through

every part of the wash in which they are immersed. These perforated pipes are not shewn in the drawing.

Mode of action.—When commencing an operation, the wash-pump is first set in motion to charge all the zig zag pipes m m m, until the wash passes over into the analyzers at n'. The pump is then stopped, and the steam let into the bottom of the apparatus by the pipe b b. The steam passes up through the chambers B B', and by the pipe z into the analyzers, from whence it descends through i i to the bottom of the rectifier at N. It then rises through the chambers k k, enveloping the zig zag pipes, and rapidly heating the wash contained in them.

When the attendant perceives, by feeling the bends l l l, that the wash has been heated in several layers of these pipes, perhaps, eight or ten layers, (but the number is not of much moment,) he again sets the pump to work, and the wash now boiling hot, or nearly so, (and always in rapid motion) flows from the pipe m at n', and passes down from chamber to chamber through the dropping pipes, in the direction shewn by the arrows in a few of the upper chambers. It may be here observed, that no portion of the wash passes through the small holes perforated in the diaphragms which separate the chambers. These holes are regulated both in number and size, so as to be not more than sufficient to afford passage to the vapour upwards under some pressure. The holes, therefore, afford no outlet for the liquor, which can only find its way down in the zig zag course indicated by the arrows. It is, therefore, obvious, that the wash as it passes down is spread into strata, as many times as there are diaphragms, and is thus exposed to the most searching action of the steam constantly blowing up through it. As it descends from chamber to chamber, its alcohol is abstracted by the steam passing through it, agreeably to the 3rd and 4th preliminary principles we have laid down, and by the time the wash has reached the large chamber B, it is in the ordinary course of the operation, completely deprived of its alcohol.

The wash as it descends from the analyzer accumulates in the upper large chamber B', until that chamber becomes nearly filled, which, when the attendant perceives to be the case, by the inspection of the glass tube, he opens the valve

of the pipe V, and discharges the contents of B' into B;
then shutting the valve, the wash from the analyzer again
accumulates in B', and, when it is a second time nearly full,
the contents of the lower chamber B are discharged from
the apparatus altogether, through the cock N, and the
charge in B' let down into B, by opening the valve as be-
fore, and thus the process goes on so long as there is any
wash to supply the pump. When all the wash is gone, a
quantity of water is let into the reservoir L, and pumped
through the pipes *m m*, to finish the process and obtain the
last portions of alcohol. This winding up of the operation
by sending water through the pipes, takes place on the
distillation of every back of wash, in consequence of an
Excise regulation, which requires the distiller to keep the
produce of each back separate from that of any other.
Were it not for this regulation the distillation would go on
uninterruptedly, so long as there was any wash in stock;
the addition of water for winding up would be necessary
but once during the distilling period, and the manufacturer
would save much time and fuel at present wasted by these
interruptions.

It has been already said, that in the ordinary course of
the operation, the wash is stripped of all its alcohol by the
time it has reached the bottom of the analyzer, but, as a
precautionary measure, the chambers B' B have been su-
peradded, in each of which the spent wash is exposed for
about half an hour to the action of the steam blowing
through it.

There is a small apparatus (not shewn in the engraving)
by which a portion of the steam in the chamber B' is con-
densed, cooled, and made to flow constantly through a
sample jar, in which is an hydrometer, or, what is better,
two glass bubbles, one of the specific gravity 1000, the other
998. The attendant knows all is right when these bubbles,
or even the lightest of them, floats in the sample. And thus,
the chamber B may be emptied without any risk of loss.

The course of the wash being understood, that of the
steam will require very little description.

The steam, as it rises, is first blown through the charges
of spent wash in the chambers B' B, thence it passes up
through the layers of wash on the eleven diaphragms of

the analyzer. In its passage it abstracts from these layers
of wash their alcohol, depositing in its place an equivalent
quantity of water. After traversing the whole of the ana-
lyzer, the vapour, now containing much alcohol, passes by
the pipe *i i*, into the bottom of the rectifier, and, as it
ascends, it envelopes the pipes *m m*, heating the wash, and,
at the same time, parting with its more watery portion,
which is condensed, and falls, in a boiling state, on the
several diaphragms of the rectifier. By the time the vapour
reaches the passage W, in the bottom of the finished spirit-
condenser, it is nearly pure alcohol, and, as it is condensed
by the wash in the pipes, and falls on the diaphragm, it is
conveyed away by the pipe *y* to a refrigerator. At the top
of the spirit condenser is a large pipe, R, which serves as a
vent for the incondensible gas which is disengaged in the pro-
cess, and this pipe also communicates with the refrigerator,
so that, should vapour at any time be sufficient to pass out
of the apparatus, no loss is sustained beyond the waste of
fuel caused by condensing that vapour by the water of the
refrigerator instead of the wash of the condenser.

The liquor condensed on the several diaphragms of the
rectifier, after being blown through by the vapour passing
up from plate to plate, descends to the bottom in the same
manner as the wash descends from chamber to chamber in
the analyzer; but this condensed liquor still contains a
portion of alcohol, and it is conveyed by the pipe S to the
pump Q, by which it is pumped up with the wash to be
again distilled.

A thermometer at *m'* shews the attendant the temperature
of the wash as it issues from the pipe *m m*, into the ana-
lyzer, which is the only guide he requires for managing the
operation ; for, when the temperature is what it should be,
nothing can go wrong in the work. Whenever the ther-
mometer indicates too high a temperature more wash should
be let into the apparatus, and *vice versa ;* the quantity being
regulated by the cock on the pipe *n*. It would seem, how-
ever, that very little nicety is requisite on this point. The
attendant finds by experience that the fluctuation of a few
degrees above or below the proper heat is of little conse-
quence; and, we observed, that he very seldom found it
necessary to alter the supply of wash.

The water for supplying the boiler passes through a long coil of pipe immersed in the boiling hot spent wash, by which means it is raised to a high temperature before it reaches the boiler. It will be seen that the vapour passing through this apparatus is all condensed by the wash, not water; and, therefore, no heat is wasted, as in the common process. The consequence of this is, that about three-fourths of the fuel used with the common stills is saved, a matter of very important consideration, in a national point of view.

According to the common process, it requires 12 lbs. of coals to distil a gallon of proof spirits,* of which, as we have said, 9 lbs. are saved by the new system; and, assuming the whole quantity of spirits distilled in the empire to be 36,000,000 gallons, which (colonies included) we believe is not over the mark, the saving of fuel arising from the new methods of distilling, which, no doubt, will be soon universally adopted, will amount to 140,000 tons of coal per annum.

Our Continental readers have no idea of the enormous size of some of the distilleries of the United Kingdom. The apparatus of Mr. Coffey, at Inverkeithing, of which we have given a description, distils 2000 gallons of wash per hour; and one which he has subsequently erected at Leith, for the same proprietors, upwards of 3000 gallons per hour. There are several of equal magnitude; and we have seen a statement, which we have reason to rely on, which shews that those now erected, or being erected, are of capacity to distil half a million of gallons of wash per day. This wash yielding, on an average, from 11 to 12 per cent. of proof spirits.

ARTICLE III.

Alcohol and its Compounds.†

1. ALDEHYDE, (from *alcohol dehydrogenatus,*) may be prepared by passing vapour of ether through a long glass tube

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* When the coals are of the best quality, the furnaces scientifically constructed, and when strong wash is used, a gallon of spirits can be distilled with much less than 12 lbs. of coals; but we have good reason to believe the average consumption is not less than that.

† Ann. de Chim. et de Phys. lix. 289.

filled with pieces of glass heated to redness. The product, according to Liebig, is aldehyde, an inflammable gas, and water, with a slight deposit of charcoal. By passing this product into a vessel, half filled with ether, the aldehyde is retained in solution. If ammonia, passed through a tube filled with fused potash and quicklime, is allowed to saturate the ether, the sides of the vessel are speedily covered with brilliant crystals, which are compounds of aldehyde and ammonia. Aldehyde may be also procured by distilling four parts of spirit of wine, six parts peroxide of manganese, six of sulphuric acid, and four of water. The receiver must be kept very cool, as aldehyde is extremely volatile. The process should be stopped whenever the product becomes acid, which happens when six parts have come over. This product mixed with its weight of chloride of calcium, is distilled to three parts. The three parts are again rectified with their own weight of the chloride, when the resulting product is free from water and alcohol. It should then be mixed with twice its volume of ether, and saturated with a stream of ammoniacal gas, taking care to cool the receiver and to place between the vessel supplying the ammonia, and the ether vessel, a safety jar, so as to avoid the danger from the rapid absorption: Crystals speedily appear, which, when purified by ether, consist of ammonia and aldehyde, and are termed by Liebig, *ammonialdehyde.* The same compound may be obtained by passing chlorine through dilute alcohol, distilling and rectifying over chloride of calcium, and saturating with ammonia. A considerable quantity of aldehyde is also formed by the action of spongy platinum upon the vapour of alcohol, as ascertained by Döbereiner. Aldehyde is easily prepared, from its ammoniacal combination, by dissolving two parts of the compound in its weight of water, and heating it, mixed with three parts of sulphuric acid and four of water, in a retort over a water-bath. The product is hydrous aldehyde, which is rectified over chloride of calcium. It is necessary to cool the vessels when these two substances are brought in contact; because, much heat is disengaged, and the aldehyde boils, when re-distilled, at a temperature of 86°.

It is a colourless liquid, limpid like water; very volatile; sp. gr. ·790; boiling point, 71°¼ at 28·82; smell ethereal

and peculiar. When its vapour is respired the power of
breathing the air for some seconds is lost. It mixes in all
proportions with water. It inflames readily. When mixed
with spongy platinum, acetic acid is formed. It dissolves
sulphur, phosphorus, and iodine, but without altering them.
chlorine and Bromine are converted into muriatic and
hydro-bromic acids. With nitric acid, acetic acid is formed;
with potash a reddish-brown resin is formed, which Liebig
designates by the awkward name of *Aldehydharz*. When
aldehyde is heated with water and oxide of silver, the latter
is reduced, and covers the inside of the tube with a metallic
coat. Aldehyde consists of

Carbon	. . .	55·024
Oxygen	. . .	35·993
Hydrogen	. .	8·983

The density of its vapour is, by experiment, 1·532, which
corresponds with

2 vols. vapour of carbon	. .	·8333 =	1·5
2 ,,	hydrogen .	·1388 =	·25
½ ,,	oxygen . .	·5555 =	1·
		1·5276	2·75

Liebig gives its formula $C^4 H^3 O$.

Ammonialdehyde crystallizes in acute rhombohedrons.
The crystals are colourless, possess a hardness equal to sugar,
and a smell like that of ammonia and turpentine; they
are volatile; inflammable; melt between 158° and 176°.
They have an alkaline re-action; they dissolve readily in
water, with greater difficulty in alcohol, and with difficulty
in ether. With the acids and alkalies they act like aldehyde
and consist of

Carbon	. . .	39·700
Oxygen	. . .	25·969
Azote.	. . .	22·987
Hydrogen	. .	11·342
		100·000

This is equivalent to

1 atom aldehyde, $C^2 H^2 O$	= 2·75
1 atom ammonia $N H^3$	= 2·125
1 atom ammonialdehyde	. 4·875

Inflammable Gas.—The gas which comes over with alde-

hyde burns with a clear flame. It consists of carbon, 82·3; hydrogen, 17·6.

When heated with perchloride of antimony (readily formed by passing chlorine through fused *butter* of antimony) olefiant gas was condensed in the form of the well-known oily chloride, and the remaining gas possessed all the properties of carburetted hydrogen.

The products of the distillation of alcohol, sulphuric acid and peroxide of manganese, are carbonic acid, formic acid, acetic acid, aldehyde, and traces of ether.

With spongy platinum alcohol is converted into acetal, aldehyde, acetic acid, and acetic ether.

Resin of Aldehyde is formed by the action of potash upon aldehyde. When the latter is introduced into a liquid containing aldehyde, a brown colour is produced, and speedily brown flocks fall, when a weak acid or water is added. They consist of carbon, 73·340; oxygen, 18·900; hydrogen, 7·759.

Aldehydic Acid.—When oxide of silver is heated with a solution of aldehyde a soluble salt is formed, which is not an acetate, and is permanent when evaporated. This salt, when mixed with barytes water, is decomposed, giving off oxide of silver, and, when heated with the salt of barytes formed, produces pure acetate of barytes, and no other products: the oxide of silver being completely reduced. A similar result is obtained by the action of ammonialdehyde upon oxide of silver.

From Liebig's experiments it appears that the formula of aldehydic acid is, $C^4 H^3 O^2$, and, therefore, a true acetous acid; the composition of acetic acid being $C^4 H^3 O^3$. He considers the lampic acid of Daniell to be identical with aldehydic acid. The combinations may be explained in two ways, according to Liebig:

1st, Aldehyde may be considered as alcohol deprived of an atom of hydrogen, and alcohol as a hydrate of ether; or, 2nd, Aldehyde may be a deutoxide of binolefiant gas. The formulæ will, therefore, be,

1st, Unknown compound of carbon
and hydrogen . . . . . $C^4 H^3$
Aldehyde . . . . $C^4 H^3 O + HO$
Aldehydic acid . . . . . $C^4 H^3 O^2 + HO$
Hydrous acetic acid . . . . $C^4 H^3 O^3 + HO$

2nd, $C^4 H^4 + O$ oxide of binolefiant gas.
$C^4 H^4 + O^2$ aldehyde.
$C^4 H^4 + O^3$ aldehydic acid.
$C^4 H^4 + O^4$ hydrous acetic acid.

ARTICLE IV.

On the Action of Flashes of Light upon rapidly Rotating Disks. By CHARLES TOMLINSON, Esq.

PROFESSOR WHEATSTONE, I believe, first announced the beautiful fact " that a rapidly moving wheel, or a revolving disk on which any object is painted, seems perfectly stationary when illuminated by the explosion of the electric jar."

This experiment is adduced by Mr. Wheatstone, to shew that the duration of electric light embraces a point of time so extremely minute that the revolving wheel, or disk, has not time to pass through any perceptible space, and that, therefore, it appears, during the illumination, stationary ; I find, however, that the effect is not confined to electricity, but may be produced by any very sudden flash of light.

Of the disks that I employed I need only mention two : The first, six inches in diameter, was divided into sixteen parts, painted, alternately, red and black ; on the second disk, of the same size, were painted in large characters the words, AT REST, on white ground. Both disks were connected with a small multiplying arrangement.

The effects can be produced with phosphuretted hydrogen, exhibited in bubbles from phosphuret of lime, in water. When the bubbles come up slowly without interrupting each other, both disks appear stationary during rotation ; but when the bubbles come up too quickly, the black and red spaces exhibit a dancing sort of motion, and sometimes two black spaces seem joined into one, to the exclusion of the intervening red, and vice versa ; so also with the second disk, the words cross each other in various directions when the flashes of light interfere with each other ; and, in both cases, confusion is, of course, excited when an impression is made on the retina before succeeding impressions have departed. Similar confused effects are produced with

a stream of electricity instead of the discharge; as also
by the rapid succession of sparks from a magnet, but in any
case when the flash of light is distinct and sudden, the
effect is complete.

Soap bubbles, blown with hydrogen or the mixed gases,
and fired by means of a filament of cotton passed through
a small tube, and wetted with alcohol; gunpowder, done
up in the form of a boy's cracker; fulminate of mercury
struck on an anvil, may all be successfully employed.

These experiments were performed in a darkened room,
not of necessity, but the results are best observed in this
manner. In Mr. Wheatstone's experiment, the presence of
light, either natural or artificial, does not interfere with its
success.

The experiment may be made to succeed by the flame of
a lamp or candle. In order to effect this I employed a
disk of pasteboard, twelve or thirteen inches in diameter,
with a narrow slit cut out, extending from the centre nearly
to the circumference, and connected with a multiplying
arrangement. The light of the lamp was condensed by a
lens, and thrown upon the back of the slitted disk, and the
black and red disk placed in the front of the former, so
as to receive a flash of light from the lamp every time
the slitted disk performed one revolution. On causing
both disks to revolve, the black and red spaces were dis-
tinctly brought out, assuming, however, a curved form.

But, perhaps, the most convenient method of producing
this phenomenon, is to stand behind the slitted disk, while
in front of it, at the distance of two or three feet, the radi-
ated disk is made to rotate. On rotating the slitted disk
the effect is very complete. The radii are, however, curved
either *upwards* or *downwards*, according as the eye of the
observer is *above* or *below* the axis of the disk, except the
radii which, for the time being, are vertical to the axis
above and below, and these are not curved. This effect
takes place when the disks are revolving in the same direc-
tion. The order will be inverted, if the disks move in
opposite directions; a change also will take place in the
direction of the curvature of the radii, according to the
angle at which the eye is placed.

This experiment is somewhat analogous to one by Dr.

Roget, " when a carriage wheel, rolling along the ground, is viewed through the intervals of a series of vertical bars, such as those of a palisade, or of a Venetian window blind. Under these circumstances, the spokes of the wheel, instead of appearing straight, as they would naturally do if no bars intervened, seem to have a considerable degree of curvature."—(Phil. Trans. 1825.)

It was found that " the velocity of the wheel must not be so great as to prevent the eye from following the spokes as they revolve." So that Dr. Roget's experiment relates simply to the curvature of the spokes of a wheel seen through a narrow aperture; and he accounts for this fact by assuming the deception to arise from separate parts only of each spoke being seen at the same moment; the remaining parts being concealed from view by the bars. He also found that " when the disk of the wheel, instead of being marked by a number of radiant lines, has only one radius marked upon it, it presents the appearance, when rolled behind the bars, of a number of radii, each having the curvature corresponding to its situation, their number being determined by that of the bars which intervene between the wheel and the eye. So that it is evident that the several portions of one and the same line, seen through the intervals of the bars, form on the retina, the images of so many different radii."

My experiment differs from that of Dr. Roget, inasmuch, that the red and black disk may be made to revolve with very great rapidity, by which the black is lost to the eye, and the red alone reflected, slightly diluted with black. The effect of viewing this disk during rotation through the rotating slitted disk, is to decompose the former, and present the black and red spaces as distinctly as when at rest, except that the spaces are curved, and, under certain circumstances, increased in number.

If a white disk be employed, with a single black space passing from the centre to the circumference, and occupying about 20° of the latter, the effect will not be as in the case of the disk of the wheel with only one spoke giving the appearance of a complete wheel, as in Dr. Roget's experiment, but the black space will be brought out in a curved form, and sometimes divided into two.

. If a disk, composed of two semi-circles, one white and the other black, be viewed, while in motion, from behind the revolving slitted disk, the diameter of the disk will vibrate on both sides, the centre being fixed; the white gaining upon the black and the black, upon the white, and so on, alternately.

The cause, then, of the appearances detailed in the first part of this paper, is the same as in Mr. Wheatstone's experiment, the light comes and goes before the disk has time to move through any sensible space; but, in the experiments where the light of a lamp flashes upon the painted disk through the slitted disk, or where the eye is placed behind the slitted disk, the duration of the light is greater than the electric light, or than that from phosphuretted hydrogen, &c., and the disk *does* pass through a sensible space. Now, as the circumference of the disk moves quicker than the centre, that is, the velocity decreases from the circumference to the centre, a black space, for example, seen at one point of the circumference, will have moved through several degrees as the slit passes the eye; while, at or near the centre, the space gone through is barely appreciable. This, together with the persistance of impressions on the retina, added to that which is said above, will, I think, account for the revival of the radii, as also for their curvature; and the rapid succession of black and red spaces will account for the apparent increase in their number.

If the distance between the two disks be considerable, fourteen or fifteen feet, for instance, the curvature of the radii will be corrected, and their number will not be augmented; because, a full view of the disk is thus obtained, and the relative velocities of the centre and circumference compensated by an impression of the whole of the disk being formed upon the retina.

Salisbury, 18*th November*, 1835.

ARTICLE V.
On Madder, and Madder Dyeing.
(*Continued, from* vol. ii. page 457.)

Characters of Madder-Purple.—When cautiously heated in a glass tube, madder-purple melts into a dark-brown viscid

liquid, from which a vapour proceeds, which does not collect in the form of needles, but as a brown-red, viscid, mass on the sides of the glass. By the addition of more heat it may be driven along the tube, but is then covered with carbonaceous matter; so that the sublimed madder-purple cannot be again sublimed without decomposition. It colours alum, and iron mordanted cotton, as well as madder-purple; and its solution in caustic potash presents a cherry-red colour. In completely pure water, madder-purple forms, with the addition of heat, a dark, pink-coloured solution. In cold water it is little soluble. A hot solution, when cooled, deposits no flocks. Acids change the pink solution to a yellow colour.

In well-water, or water containing lime, the madder-purple dissolves at first; but a portion combines with the whole lime, and precipitates in the form of a dark-red gum. The colours to be produced by madder-purple must be communicated by means of water, perfectly free from lime, else the loss of colouring matter will be very great.

Spirit, *alcohol*, and *ether*, dissolve madder-purple very readily, and form orange-yellow solutions. After the evaporation of the liquid, the madder-purple remains, in the form of a bright, orange-yellow, crystalline powder. When water is added to a hot, concentrated solution of madder-purple, in spirit, a quantity of silky crystals is separated, which swim in the solution.

Dilute acids, at the boiling temperature, dissolve madder-purple, forming a yellow solution; on cooling, it separates in the form of orange-yellow flocks.

Ammonia forms, with madder-purple, a beautiful bright-red solution, which, when printed upon unmordanted cotton, and, after drying, being washed in hot water, leaves a clear pink colour. When printed upon cotton with alum mordant, and washed in boiling water, a clear red is obtained.

Solution of potash dissolves madder-purple, forming a fine cherry red-coloured solution, and gives, upon unmordanted cotton, after clearing with hot water, a pink colour. On mordanted cotton a saturated dark-red colour is obtained in the same circumstances.

The solution of madder-purple, in spirit, imparts to unmordanted cotton a pink colour, which is reddened by

alkalies. Cotton impregnated with the alum mordant produces, at a boiling temperature, a different colour according to the quantity of purple added :

One part purple to 16 cloth affords a dark reddish-brown
,,   ,,   40   ,,   a saturated purple-red
,,   ,,   80   ,,   a saturated bright-red.

Clay, when added to the liquid, modifies these colours, and makes them brighter, and more of a scarlet shade, in consequence of the loss of colouring matter by the formation of a red gummy matter. When 240 parts clay are employed with 1 purple and 40 cloth, the shade is only half as dark as when no clay is used.

The presence of chalk is decidedly injurious. When one part of madder-purple is boiled with one part chalk, in a great quantity of water, no precipitate is formed, but a bright-red solution is produced. Presently a red gummy matter is deposited on the sides of the vessel. If the quantity of chalk is gradually increased, a point is attained where all the madder purple separates from the chalk, and is converted into red gum. The liquid has now lost its power of dyeing. The presence of chalk, in dyeing with madder, is also prejudicial as with madder-purple. This effect is modified in the case of madder-red, where the chalk combines by preference.

With cotton which is oiled and mordanted for the reception of the Turkey-red dye, the madder-purple does not produce a true Turkey-red. It possesses too much blue ; and, in order to acquire the first, it must be subjected to the usual clearing operations. Without this it is very light.

With tin, lead, and iron mordants, the madder-purple produces colours which pass from red, through brown-red and brown, into blue. The colour with tin mordant is *pink;* with lead mordant, *scarlet;* with copper mordant, *red-brown;* and lastly, with iron mordant, *violet.* All these colours receive their full impress by the employment of a sufficiently strong mordant.

By soap, carbonate of soda, and clay, neither the dark nor the bright red of the madder-purple is changed. Still, soap produces an injurious effect when an excess is employed. If the dark dye is boiled for a quarter of an hour with 1 part of soap, 3 cloth, and 240 water, the dye

loses some of its lustre, and becomes brighter, while the soapy water is coloured red. The same effect is produced on the bright dye.

Carbonate of soda, in the proportion of 1 part soda to 8 cloth and 240 water, acts advantageously when boiled for a quarter of an hour. With the dark colour the solution becomes slightly red, but on the bright colour no effect is produced. The shades are not perceptibly changed. Clay has no injurious effect upon either colour. If we take 1 part cloth, 3 clay, and 240 water, and boil for a quarter of an hour, the clay removes no colour, and the solution, at the utmost, is only slightly tinged red. The shades are not perceptibly altered. From these results, it is obvious that the combination of madder-purple with alumina forms the common madder-purple, or Turkey-red; and that it also enters, as the principal constituent, into common madder-red. Soap, soda, and clay, rather tend to render the colour clearer. Light, also, after the action of a July sun for 60 hours, makes it brighter.

Madder-red.—The discrimination of madder-red, and its separation from madder-purple, depend upon its insolubility in a strong alum solution. When madder has been washed according to the process recommended for separating madder-purple, if it be then boiled with alum solution, a brown-red precipitate separates, containing much madder-red. From the precipitate it may be separated by boiling the latter frequently with strong muriatic acid, washing it well, and treating it with boiling spirit of wine. This gives a dark-brownish red coloured tincture, which, after the evaporation of the acid, and cooling, deposits an orange-yellow precipitate. When washed with cold spirit it consists of madder-red, which is mixed with much madder-purple. The latter is separated by boiling it with a solution of alum, which is to be repeated as long as the liquid is coloured red. When both colouring matters have a resinous consistence, the action of the alum must be diminished, previously dissolving them in a little spirit, and then adding the alum solution. When the point is attained at which the alum solution remains colourless, and no more madder-purple is left, the yellow precipitate is to be edulcorated, dried, and dissolved in ether. The ethereal solution being evaporated, the

madder-red is obtained in the form of a brownish-yellow crystalline powder.

Characters of Madder-Red.—These, as far as the dyer is concerned, are as follow : When carefully heated in a glass tube, madder-red melts into a dark orange-coloured liquid, and, leaving some charcoal, is driven off in the form of a yellow vapour, which condenses into bright orange-coloured needles. By additional heat, the sublimate may be driven along the tube without leaving any carbonaceous residue, so that the first sublimed portion is not decomposed but is capable of a second sublimation. It colours alum and iron mordanted cotton, as well as madder-red itself. The red has more of a fire colour (mehr feuer) than that of the unsublimed portion. Sublimed madder dissolves in potash ley with a blue colour.

In *pure water* the madder-red dissolves by heating, and forms a dark-yellow solution. It is difficultly soluble in cold water. A hot solution, on cooling, allows the madder-red to precipitate partly in the form of orange-yellow flocks. Acids change the dark-yellow solution into a bright-yellow.

In *well-water*, and *water containing lime*, madder-red dissolves with a purple-red colour, a blueish gum being formed. Portions of alum-mordanted cotton, therefore, exhibit no medium-red colour, but a dark, red-brown purple colour; chalk acts in the same way, which affords a sufficient distinction from madder-purple.

Spirits, alcohol, and ether dissolve madder-red, forming a reddish-yellow solution. After the evaporation of the solution a brownish-yellow crystalline powder remains. When water is added to a hot concentrated solution of madder-red in alcohol, a quantity of silky crystals separate, which swim in the liquid.

Dilute acids dissolve madder-red, forming a yellow solution. On cooling orange-yellow flocks separate.

Ammonia forms, with madder-red, a beautiful purple-red solution, which, when printed on unmordanted cotton, and washed, after drying in hot water, leaves a dark-red colour without lustre. When printed (on cotton which has imbibed the alum mordant) and then washed in boiling water, a dull red is obtained.

Potash ley dissolves madder-red, producing a *beautiful*

violet-blue solution, which, by an excess of madder-red, passes into purple. For printing it gives no better results than the solution in ammonia. To unmordanted cotton the solution in spirits imparts a yellow rust colour.

By printing with caustic alkalies, especially barytes, clear lilac colours are obtained, which have no permanence.

To the aluminous mordanted cotton, madder-red imparts a dark-red colour, without brightness (feuer) and beauty.

For 28 parts of cloth 1 part madder-red is sufficient for saturation. If more is used the colour is not darker, and the excess remains in the vat. The addition of clay is of decided benefit. It makes the colour considerably darker, and redder. The best proportion is to take ⅙th of the weight of the cloth, or, 132 parts clay, 1 madder-red, and 22 cloth.

The addition of chalk to madder-purple is very injurious, but to madder-red is advantageous.

If 1 part madder-red, and 1 part chalk, are boiled with a sufficient quantity of water, the solution previously yellow becomes dark purple-red; and 22 parts of alumed cotton acquire a colour which is brighter than that of the cotton saturated with madder-purple.

More chalk, as, for example, about as much again, affords also a good result. In general, however, a greater quantity is injurious, as the colour becomes brighter than with 1 part chalk, and the gum which is formed is redder.

This remarkable action of chalk upon madder-red explains the advantage which is gained by adding chalk in dyeing with certain kinds of madder.* In these it is evident that madder-red is the principal constituent. A remarkable distinction is thus also made between madder-purple and madder-red, viz. That the addition of chalk to the first is prejudicial. It is, therefore, necessary to determine the proportion of madder-red in the species of madder employed.

Still more remarkable is the action of chalk upon madder-red in dyeing cotton which is *oiled and mordanted* for the Turkey-red dye. Without the addition of chalk the last acquires a dull, dirty, brown-red colour. With chalk a fine Turkey-red colour is procured, without *clearing*. When

* See Schlumberger and Robiquet's Explanation of the Effect of Calcareous Matter on Madder, Records of General Science, i. 207.—EDIT.

compared with the best Turkey-red it differs, in so far as
that it does not present the pink or blueish mixture which
madder-purple forms with the mordanted oiled cotton (ge-
beizten oelkattun).   When both shades, the scarlet-red
and the purple-red, are found mixed in Turkey-red, there
is then a double compound formed with the madder-purple,
madder-red, and mordanted oiled cotton.   The different
shades may be mixed at pleasure; for, if we wish to give
more scarlet, a little more madder-red should be added; if
a greater tinge of purple is required, an addition of madder-
purple will answer the purpose.

It has been already remarked, under madder-purple,
that 160 parts of oiled cotton will be dyed of a clearer,
darker, and more complete colour, by 1 part of madder-
purple, than the half, or 80, of common alum-mordanted
cotton.   With madder-red the proportion is still more im-
portant. If we use double the quantity of dye, viz. 44 oiled
cotton, 1 madder-red, 1 chalk, we obtain a colour which
is not so intense as a colour produced by 44 alum-mor-
danted cotton, with a double portion of madder-red, where
a considerable quantity of red gum is formed in the vat.

With tin, lead, and copper mordants, madder-red affords
ugly colours, viz. reddish-yellow, brownish-red, and
brownish-violet. With iron mordant, however, by using
a strong solution, and then washing the cloth, a beautiful
lilac-violet colour is formed, which becomes very dark by
the addition of chalk.

It is the madder-red which forms the *violet* and *lilac*
colours obtained by means of Avignon madder and strong
iron mordant. The violet of madder-purple is also fine, but
it requires the employment of blue clay (blaue thon) which
distinguishes it from madder-red violet.   Hence, therefore,
alumina is the proper mordant for madder-purple, while
the last gives a beautiful red without addition.   So is it the
oxide of iron for madder-red in relation to *lilac-violet*, which
distinguishes both in combination.

Soap, carbonate of soda, and clay, act differently upon
madder-red, according as it has been dyed with or without
chalk.   Cloth dyed without chalk is acted on by soap, in
the proportion of 1 soap to 3 cloth, becomes pale, and loses
much of its lustre; while cloth dyed with chalk, even after

long boiling, loses scarcely any thing, although the soapy water is tinged reddish. Carbonate of soda, in the proportion of 1 to 8 cloth, acts upon both kinds of red advantageously. The red without chalk it makes slightly redder; but it withdraws much colour, as the tinging of the liquid shews. The red with chalk becomes more vivid, the alkali scarcely extracting any colour. When 3 parts clay are boiled for a quarter of an hour with 1 part of cloth in 240 water, no action takes place on either of the reds.

The action of the sun in July, during an exposure of 60 hours, produces even less effect upon madder-red dyed with chalk than upon madder-purple red. Madder-red, when used as a dye, is characterized by its combination with alumina, in the absence of any additional substance, being a dull, useless colour; but when, on the other hand, clay or chalk is added, a fine saturated red is produced.

Madder-orange.—The distinction of madder-orange, and its separation from madder-purple and madder-red, depend upon its little solubility in spirits. To separate the madder-orange in a state of purity, a cold infusion *(aufguss)* of Alizari, at 59° F. should be prepared. The latter should be carefully edulcorated, washed well with pure water, then digested with eight times as much water, and macerated for 16 hours. The brown-coloured infusion should now be strained through muslin, and its place supplied by fresh water. This should remain for 16 hours in contact with the alizari; it should then be strained and mixed with the first infusion. After four or six hours repose the liquid should be poured off from the sediment, and the madder-orange separated by filtering through fine paper. The liquid exhibits, on being stirred, a quantity of small crystals of madder-orange, which remain on the filter. These should be well washed with cold water, afterwards boiled with spirit, and the solution filtered while hot. From this solution madder-orange precipitates on cooling, which is to be washed with spirit until it dissolves in sulphuric acid with a fine yellow colour (without mixture of red). When this dye possesses still a reddish-colour, it is a proof that the orange is mixed with madder-purple, or madder-red. A still surer proof of the purity of the madder-orange is derived from the circumstance of its imparting a nankin colour,

without a trace of red, to cotton impregnated with the tin mordant.

Properties of Madder-Orange.—When heated in a glass tube, madder-orange exhibits the same characters as madder-purple, but, with this difference, that the vapours disengaged are yellow, and condense into a yellow-brown mass. If this is heated again, the same character is exhibited as with madder-purple; some charcoal is left. So that, also, in this case, the matter sublimed once, cannot be again sublimed without decomposition; in other respects it dyes cotton impregnated in the alum and copper mordants, as madder-orange itself. It forms also a yellow solution with sulphuric acid.

Thus, three colouring matters in madder may be sublimed without undergoing any essential change, like indigo, when it is subjected to an equal temperature. By sublimation, alone, therefore, it is impossible to separate the three colouring matters. *Alizarine* must, therefore, be a mixture consisting of more than one substance.

With *pure water*, madder orange, by the addition of heat, forms a yellow-coloured solution. On cooling, some deposition takes place. In cold water the colouring matter is little soluble. In well-water, or water containing lime, madder-orange, by the addition of heat, forms a reddish solution; and its dyeing power will, in consequence, be diminished, or altogether destroyed, as the quantity of water is increased.

Ether dissolves madder-orange readily. By evaporation it remains in the form of a bright-yellow crystalline powder. Cold spirit dissolves it sparingly; boiling spirit forms a bright-yellow solution, from which, on cooling, the greater part of the madder-orange separates. If water is added to a hot solution in spirit, small crystals separate, as with madder-red and madder-purple, under similar circumstances.

Dilute acids form, with madder-orange, a yellowish-coloured solution; on cooling, the greater part separates.

Liquid ammonia forms a red-brown solution, from which, on evaporating the ammonia, orange-yellow flocks separate. When printed on the alum-mordanted cotton a dull orange-colour remains, after washing in hot water.

Potash ley forms, with madder-orange, a dark red-coloured

solution, which changes by the access of light into orange. When printed upon cotton impregnated with alum-mordant the result is not superior to that with the ammoniacal solution ; 30 parts of mordanted cloth require for saturation only 1 part of madder-orange. It requires, therefore, the greatest quantity for saturation of all the madder dyes; madder-purple requiring only 16 and madder-red 22 of cloth for saturation. Madder-orange affords a clear combination with cotton when it is quite pure. Hence, the addition of clay and chalk is injurious. When a certain quantity of clay is added with the cloth, the colour is brighter, but when the quantity is increased the colour becomes reddish orange.

The remarkable action of the clay now points out the reason of its efficacy in dyeing with madder. With madder-red, clay strongly reddens its yellowish-red combination with alumina. Here the clay is the cause of the alteration, and while added in proper proportion, it prevents the direct brightening of madder-orange. The reason is that the clay has more affinity for the madder-orange than the aluminous mordanted cotton: If we examine the clay employed, we find that it has usually a bright orange-colour, and becomes, by digestion in caustic potash, as red as the madder-orange itself. In like manner, the dye solution employed is reddened by potash. The colouring matter is, therefore, disguised *(zurückgehalten)* by both. But, since madder-orange gives a shade of yellow to the red colour of the madder-purple and madder-red, it is evident that the presence of clay makes the colour more red.

The addition of chalk produces the same effect as with madder purple: If 1 orange, 30 cloth, and 1 chalk, are used, the colour will not be half so dark as without chalk. Much gum is thereby separated. The colour thus produced is more rapidly deteriorated by light than one which is formed without chalk.

With copper mordant, an orange colour is formed. Cotton impregnated with the lead mordant, acquires a reddish rust colour, and iron mordanted cotton is coloured strongly *nut-brown.* With the tin mordant upon cotton a bright *yellow-nankin* is obtained. We can readily distinguish in the last dye, whether the madder-orange is free from madder-

purple or madder-red, as the tin mordant forms with these, reddish compounds. When boiled with soap, madder-orange is deteriorated. It loses its lustre, and becomes dull and reddish. Carbonate of soda (1 to 8 cloth) produces a similar, though less intense colour. In both cases, the solutions are coloured yellow. By boiling with clay, the orange loses some of its yellow lustre and becomes lighter. Light injures this colour. Exposure to a July sun for 60 hours, causes it to lose the half of its colour. A specimen dyed with chalk is deteriorated still more rapidly. From the preceding observations, it appears, that the best mordants for madder-orange as a dye, are alumina and oxide of copper.

Madder-yellow.—The distinction of this dye chiefly depends upon its great solubility in water, and its want of disposition to combine with cotton dipped in a solution of alum. The Dutch madder, is especially, rich in madder-yellow. It may be separated by digesting 1 part of Dutch-madder with 16 water, boiling the solution after 12 hours, and mixing it with an equal volume of lime water. In 12 hours a dark red precipitate is formed, which besides madder-yellow, contains the other constituents of madder, especially, madder-orange and madder-purple. To separate these, an excess of acetic acid is added to the precipitate, which dissolves the lime and madder-yellow, and leaves a red mass which is separated by filtration. The madder-yellow mixed with the acetate of lime is still rendered impure by the presence of some madder-purple. This is separated by boiling the solution with cotton impregnated with alum mordant, as long as it is coloured red or orange. A point is at last attained, where the cotton acquires a bright rust colour, and the yellow liquid on evaporation, leaves not a brown-red, but a bright yellow residue; the colouring matters are then completely separated. The yellow residue is now dissolved in spirit, and the madder-yellow precipitated from its solution, by means of an alcoholic solution of acetate of lead. A scarlet-red precipitate falls, which is to be edulcorated with spirit, then dissolved in water, and precipitated by sulphuretted hydrogen, by which means the madder-yellow is separated from the oxide of lead. Since, cotton impregnated with alum mordant acquires only a dull nankin colour, by the addition of

madder-yellow, and is a very inferior kind of dye, its other properties may be omitted. The other constituents of madder require no attention here, because they are totally useless as dyes. But as they (especially Rubiacic acid) may interest chemists and botanists, an account of them will be reserved for Poggendorff's Annalen.

Criticism on Analyses of Madder.—No analyst has hitherto obtained any of the preceding colours in a pure state. All the substances described under the names of Extractive, Woody madder-red, Erythrodanum, Alizarine, red colouring matter of madder, pink colouring matter of madder, and Xanthine, were indefinite mixtures of madder-purple, madder-red, madder-orange, and madder-yellow.

The extractive madder-red of Bucholz, is an extract formed by means of water and spirit; it contains, therefore, such constituents of madder as are soluble in both. The madder-red of Kuhlmann, formed by precipitating an aqueous infusion of madder by means of sulphuric acid, contains both of the red colouring matters in madder, and besides madder-yellow which is also thrown down by sulphuric acid. In Robiquet's Alizarine obtained by sublimation from the carbonaceous matter of madder, madder-purple is finer than any of the preceding, but it is mixed with madder-red, as appears by the re-action with potash ley which is not pure cherry-red but purple red, from the mixture of blue which potash forms with madder-red.

The two colouring matters which Gaultier de Claubry and Persoz have separated from madder, are mixtures, principally, of madder-red and madder-purple. One of them, termed the *red principle*, is separated by carbonate of soda in a hot solution, and precipitated by an acid. It is a reddish-brown matter with a splendent fracture. This is obviously a mixture of madder-brown and madder-yellow. But all the three pigments may be contained in this brown matter as carbonate of soda dissolves them very readily, and also takes up the madder root. The fact of its insolubility in alum shews the presence of madder-red.

The second colouring matter, they term *the pink-red principle*. It corresponds in many respects with madder-purple. Its action with solution of alum and sulphuric acid is the same, but its other characters, a compact mass with

a fracture like a drop of gum, which by pulverization be-
comes pink-red, shew that it is not madder-purple, which
is an orange-yellow crystalline powder, dissolving in potash
and forming a cherry-red solution; therefore, the violet
colour described by Claubry and Persoz, indicates the in-
termixture of madder-red. Under the name Xanthine or
madder-yellow, Kuhlmann has described a constituent of
madder, which from its properties must be a mixture of
madder-orange and madder-yellow. It dissolves readily
in water, and forms with mordanted cotton a Pomeranian
yellow. This easy solubility indicates madder-yellow, and
the Pomeranian colour the madder-orange, and was re-
marked in separating the latter from madder-yellow, when
brought in contact with cotton impregnated with alum.
Madder-yellow so prepared, dyes no longer orange. The
pure madder-purple was described by Runge in 1823.

(To be continued.)

Article VI.

*On the method of determining the proportions of Potash and
Soda, when the two alkalies are mixed together.* By
Thomas Thomson, M. D., F. R. S. L. and E., &c., Regius
Professor of Chemistry in the University of Glasgow.

It is no uncommon thing to meet with minerals which con-
tain both potash and soda as constituents. This is the case,
for example, with glassy felspar, couzeranite, &c. The
method of separating the two alkalies from each other, in
such cases, is that first pointed out by Dr. Wollaston. All
the other constituents of the mineral being separated, the
potash and soda are united to muriatic acid, or converted
into chlorides of potassium and sodium. These chlorides
being dissolved in water, are mixed with a solution of chlo-
ride of platinum. The mixture is evaporated to dryness in
a gentle heat, and then digested in a sufficient quantity of
weak alcohol. The chloride of sodium, and any excess of
chloride of platinum that may have been added are dis-
solved, while the potassium-chloride of platinum remains
undissolved. Separate it by the filter, wash it and dry it; the
potash contained in the mineral amounts to $\frac{3}{13}$ths, or 0·23

of the weight of this double salt. The weight of the potash
being known, and likewise the weight of the two chlorides
of potassium and sodium, it is easy to deduce that of the
soda.

I consider the following method easier than this, espe-
cially when the quantity of potash and soda to be separated
is considerable, and I have found that young analysts learn,
very soon to employ it with accuracy.

1. Convert the mixture of potash and soda into sulphates,
render these sulphates anhydrous by ignition in a platinum
crucible, and determine their weight. Let it amount to
29 grains.

2. Dissolve the two sulphates in water, and throw down
the sulphuric acid by chloride of barium. Wash the sul-
phate of barytes obtained, dry it and weigh it after ignition.
Let the weight be 43·5 grains, indicating 15 grains of sul-
phuric acid.

3. Separate any excess of barytes that may have been
added to the liquid by the cautious addition of dilute sul-
phuric acid. Filter, evaporate to dryness and ignite. The
salt thus obtained will consist of the mixture of potash and
soda converted into chloride of potassium and sodium.
Weigh this salt. Let the weight be 24·5 grains.

Now, the atom of potash is 6, and that of soda 4: and
it is obvious from paragraphs 1 and 2 that the mixture of
potash and soda weighs 14.

Let the atoms of potash in the mixture be x, and those of
soda y, it is plain that we have

$$6\,x + 4\,y = 14 \text{ and } x = \frac{14\cdot4y}{6}$$

By comparing paragraphs 3 and 4, it is obvious, that the
weight of chlorine in the 24·5 grains of the mixed chloride
obtained is 13·5 grains. For it must be equivalent to the
15 grains of sulphuric acid. In this mixed chloride the
potash is converted into potassium, and consequently its
atom weighs only 5, while the atom of sodium weighs 3.
We have, therefore

$$5\,x + 3\,y + 13\cdot5 = 24\cdot5 \text{ and } x = \frac{11{-}3y}{5}$$

If we equate these two values of x we have

$$\frac{14 - 4y}{6} = \frac{11 - 3y}{5}$$

By solving this equation, we obtain $y = 2$. From which we deduce $x = 1$.

Thus, it appears, that in the supposed mixture there were 6 grains of potash and 8 grains of soda.

The numbers in the preceding example were made as simple as possible, that the nature of the process might be understood at a glance. But it may be worth while, for the sake of those analysts who are not familiar with algebraic computations, to give a general formula, and then explain it by simple arithmetic.

Let the atoms of potash be . . . x

,, ,, soda . . . . . y

Let the weight of sulphates be . a

,, ,, sulphuric acid b

,, ,, chlorides . . c

,, ,, chlorine . . d

$$y = \frac{5a + 6d - 5b - 6c}{2}$$

$$x = \frac{a - b - 4y}{6}$$

Add together five times the weight of the sulphates and six times the weight of the chlorine. From this sum, subtract five times the weight of the sulphuric acid and six times the weight of the chlorides. Divide the remainder by two; the quotient represents the number of atoms of potash in the mixture. This number multiplied by six gives the grains of potash present.

If we subtract the weight of the potash from the weight of the mixture of potash and soda, determined by paragraphs 1 and 2, the remainder will be the weight of the soda present in the mixture.

ARTICLE VII.

On the Number and Character of the Colours that enter into the Composition of White Light. By PAUL COOPER, Esq.

(Continued from vol. ii. p. 365.*)*

SIR ISAAC NEWTON proved, upon mathematical principles, that when a prism is fixed in the position in which the coloured image of the sun becomes stationary, and the re-

Fig 3

Fig 1

Fig 2

V. after six reflexions

1. V
r. g
4 V after three reflexions

Fig 4.

4 V. after three reflexions
V
1.r.g
V. after six reflexions
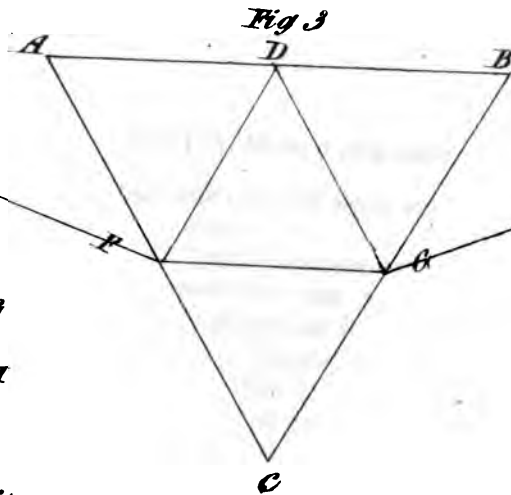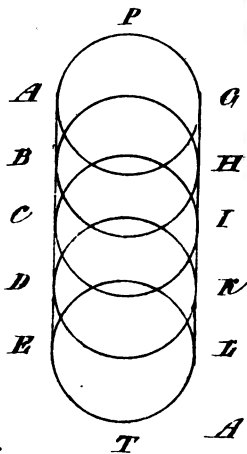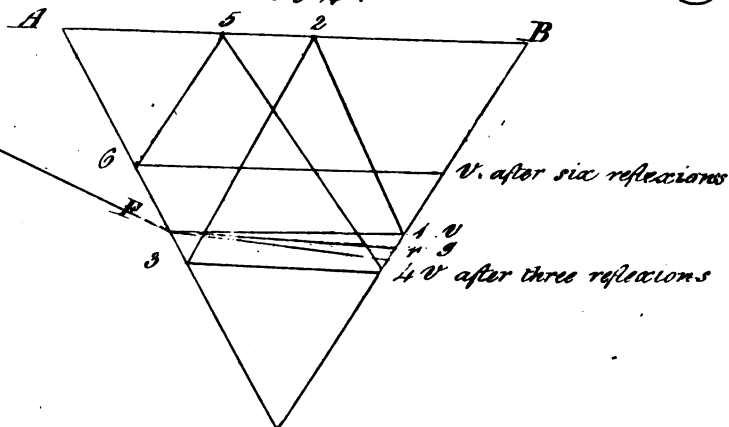
Fig 5
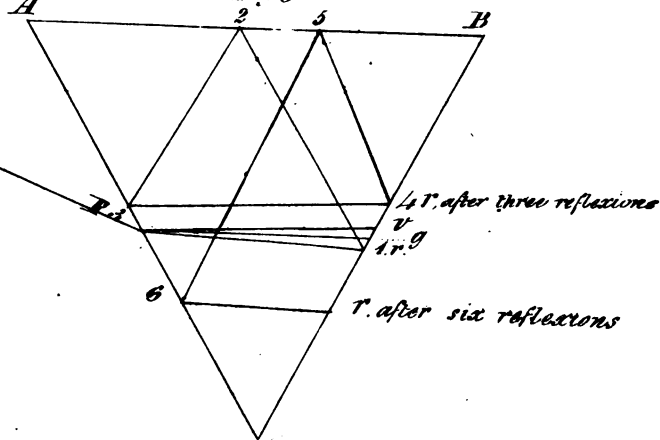
fracted light is suffered to fall perpendicularly on a screen, the figure of the image, if all the rays were equally refracted, ought to be round. Finding, then, by experiment, that this was not the case, and that the length of the image greatly exceeded its breadth, he concluded, that the rays of light are unequally refracted at the same angle of incidence.

He says, that, "By the mathematical proposition above mentioned, it is certain that the rays which are equally refrangible do fall upon a circle answering to the sun's apparent disque. Now, let A G, *(Fig. 1.)* represent the circle which all the most refrangible rays, propagated from the whole disque of the sun, would illuminate and paint upon the opposite wall if they were alone; E L the circle which all the least refrangible rays would in like manner illuminate if they were alone; B H, C J, D K, the circles which so many intermediate sorts would paint upon the wall, if they were singly propagated from the sun in successive order, the rest being intercepted; and conceive that there are other circles without number, which innumerable other intermediate sorts of rays would successively paint upon the wall, if the sun should successively emit every sort apart. And seeing that the sun emits all these sorts at once, they must altogether illuminate and paint innumerable equal circles; of all which, being according to their degrees of refrangibility placed in order in continual series, that oblong spectrum is composed which was described in the first experiment." (Newton's Optic's, p. 31.) See Smith's Optics, Article 174.

Again he says, "The solar image P T formed by the separated rays in the fifth experiment, did in the progress from its end P, on which the most refrangible rays fell, into which its end T on which the least refrangible rays fell, appear tinged with this series of colours; violet, indigo, blue, green, yellow, orange, red, together with all their intermediate degrees in a continual succession perpetually varying : So that there appeared as many degrees of colours as there were sorts of rays differing in refrangibility."— (Newton's Optics, p. 106.) See Smith's Optics, Art. 178.

It is evident from these quotations, that Newton not only considered the different colours differently refrangible, which he very satisfactorily proved; but, also, that each of the colours, into which he divided the spectrum, had innumerable different degrees of refrangibility, and shades of colour;

so that one colour, according to this hypothesis, ought to run imperceptibly into another.

These views have, I believe, been adopted, both by the advocates of the material and the undulatory theories of light; and I am not aware that any question has arisen as to their correctness.　But notwithstanding the high authority upon which these opinions rest, and the general consent with which they have been received, there does not appear to me to be any sufficient foundation for them; with the exception of the continuous form of the spectrum, which may be otherwise accounted for, they derive no support from the phenomena they are intended to explain; and some of Newton's experiments are so directly opposed to them, that I shall quote two of these experiments as the foundation of opinions precisely the reverse.

He says, " Homogeneal light is refracted regularly without any dilation, splitting or shattering of the rays, and the confused vision of objects seen through refracting bodies by hetrogeneal light, arises from the different refrangibility of several sorts of rays.　This will appear by the experiments which follow.　In the middle of a black paper I made a round hole about a fifth or a sixth of an inch in diameter. Upon this paper I caused the spectrum of homogeneal light described in the former article, so to fall that some part of the light might pass through the hole in the paper.　This transmitted part of the light I refracted with a prism placed behind the paper, letting this refracted light fall perpendicularly upon a white paper two or three feet distant from the prism, I found that the spectrum formed on the paper by this light was not oblong, as when it is made by refracting the sun's compound light, but was, (so far as I could judge by my eye) perfectly circular, the length being nowhere greater than the breadth; which shews that this light is refracted regularly without any dilation of the rays; and is an ocular demonstration of the mathematical proposition before mentioned."—(Newton's Optics, p. 62.)　See Smith's Optics, Art. 176.

The following experiment, mentioned by Newton, is still more decisive :

" A circular piece of white paper A, (*Fig.* 2) about one inch in diameter, was placed before a black wall, and using

the two prisms mentioned in a former experiment, the paper A was illuminated at the same time with the red light from the one, and a deep violet light from the other. By this mixture the paper assumed a rich purple colour. The circle A was then viewed through a prism at some distance, and the appearance exhibited was two circles, R & V, the circle R, nearer to the paper being *red*, and the more remote one, V, *violet*. The prism in this case refracted the red and violet light, mingled in the circle A, through different angles; the red being least refrangible was removed to R, and the more refrangible violet light carried so far as V.— (See Popular Account of Newton's Optics, published by the Society for the diffusion of Useful Knowledge, Section 29.)

I am aware that Newton calls the light with which these experiments were made homogeneal; but, as it was separated from white light by the prism, in the usual manner, it could not have been so, more than the other part of the same colour from which it was taken, and with which it must have corresponded; if, then, one of the prismatic colours be homogeneal, the rest must be the same.

If, in each colour, the different degrees of refrangibility were innumerable, no means could be adopted which would render any breadth of refracted light, however small, homogeneous; but in the experiment last quoted, in which it does not appear that any particular precaution was taken with this view, the illuminated paper was an inch in diameter, and yet the action of the prism produced no elongation, which must have been the case if the light employed had possessed different degrees of refrangibility.

Several other experiments of Newton's might be quoted in opposition to this part of his own theory; indeed, he so frequently neglects his particular views on this part of the subject, that if they were not so expressly stated at the commencement, it might be doubtful which side of the question he meant to support. I am persuaded that many persons, who having derived their knowledge of optics from our popular treatises, which allude to this important distinction in a very cursory manner, are not aware of its existence; so unnecessary is it to the explanation of the experiments usually given in these works, that its introduction would clog it with difficulties instead of removing them.

If we look to the spectrum itself, in the different stages of its developement, every appearance indicates one degree of refrangibility for each colour, and an uniformity in it throughout. Where, and in which of these stages, do we find that blending of colours which must necessarily arise from the superposition of innumerable circles of colours gradually differing from each other. So far from there being the slightest indication of this, every appearance is opposed to it; the two extreme colours, violet and red, preserve the same colour from their first appearance to their full developement; the blue and yellow are equally unchangeable throughout their different stages to their final disappearance; and the first line of green light, exhibited by the separation of the violet and red images, is of the same colour as when the image is fully developed.

Now, all these appearances, would be very different if the spectrum were formed of innumerable circles differing gradually in refrangibility and colour; the extension of the spectrum would withdraw these circles in succession, from the extreme colours; but beyond these, on both sides, the combinations would be of the most complicated character, and the developement of any distinct colours, except at the very extremities of the spectrum, would be impossible.

If we direct our attention to the coloured fringes, which formed one of the subjects of my last paper, we shall find the most uniform appearance in the different colours; and as we have it in our power to produce a great breadth of these colours, by looking at distant objects through a prism, we cannot easily be mistaken in their appearance.

It may be said, admitting there are only three colours, these colours, though uniform in other respects, may have different degrees of refrangibility. It must be confessed, that this hypothesis would be much more consistent with the appearance of the colours in the spectrum, and it would equally well account for its termination by two rectilinear and parallel sides; but the experiments of Newton, before quoted, are totally inconsistent with it.

Newton has demonstrated mathematically, as well as experimentally, that circular bodies, whether luminous or illuminated, preserve their round appearance when viewed through a prism, provided the light be homogenious; and

it is equally demonstrable, that with heterogeneous light, gradually and imperceptibly differing in refrangibility, the images of such bodies, formed in the same manner, must necessarily be elongated : the blending together of innumerable circles, constantly separating, but which no distance, within the limits of our experiments can separate, must produce images of this description.

If, then, we can produce by means of the prism, circular images of circular objects, as in the experiments of Newton, whether in one or more colours, we may conclude, that the light by which these images are formed, taking each image separately, is homogeneous.

The observations which I have here introduced, and various other considerations, long since convinced me that, there are only as many degrees of refrangibility as there are colours; and these I have endeavoured to show, are limited to three; but in making experiments upon the absorbtion of the light of the sun, refracted by a prism, by means of coloured glasses, I discovered images of the luminous body, which could not be thus accounted for; and, from their general appearance, it occurred to me, that these images were formed by rays of light which had undergone various reflexions within the prism.

In pursuing these and other experiments, particularly those mentioned at the close of my last paper, I discovered that the absorbtion of light by a coloured medium, when the quantities were varied in different experiments, was not in proportion to the quantity incident upon its surface, but that it bore a higher ratio in weak, than in strong light :* at least, what is sufficient for my present purpose, weak light is rendered invisible by an absorbing medium, which, under circumstances in other respects similar, transmits stronger light in quantities sufficient to form distinct images.

Upon making this discovery, which in itself, perhaps, may be of little consequence, it struck me that the images before mentioned, if formed of reflected light, would be wholly absorbed by coloured media, while so much of the direct light might be transmitted, as would give correct images of the luminous body in the primitive colours.

A great variety of experiments fully confirmed these views; and I have now no great difficulty, with a good

* This must be considered merely a hypothesis.

prism, in obtaining at the same time, circular images of the sun in red, green, and violet light, in positions agreeing with the known difference in the refrangibility of these colours, perfectly free from any intermediate images.

My method of making these experiments is, by suffering the direct light of the sun to fall on a prism, and then interpose such coloured glasses between the prism and the eye, upon which the spectrum is received, as are found best calculated to absorb the weak reflected images, and leave the primary images of sufficient strength to be distinctly visible. The glasses which succeed best are a combination of blue,* violet, and yellow, or rather pale orange ; but as the intensity of the light of the sun is different at different times, the reflected images vary in strength, and different thicknesses of the coloured glasses are required to absorb them ; no combination of glasses can therefore be specified, which will at all times insure success.

The greatest difficulty in making the experiment arises from the want of absorbing media, which will act distinctly upon the different colours ; I have, however, made the expement at least fifty times, and never attempted it, after the first discovery, when the sun was visible for a sufficient time, without succeeding. The red image is the most readily formed ; next to this, the green ; but the violet, although the orange glass gives such a complete command of this light, is sometimes attended with greater difficulty, and, to render the colour distinct, generally requires to be reduced to a faint image. The three circular images are separated considerably from each other, but not equally, the violet being much further removed from the green, than the green from the red : in the various experiments I have made, I have never observed the slightest difference in the arrangement I have now described.

(To be continued.)

ARTICLE VIII.

ANALYSES OF BOOKS.

Outlines of Mineralogy, Geology, and Mineral Analysis. By THOMAS THOMSON, M.D., F.R.S., &c. 2 vols. London, 1836.

THERE is not any more important result which has emanated from the discovery of the atomic theory than the demonstration that the

* More properly, from the light they transmit, violet, and crimson.

mineral kingdom consists, not of a multitude of heterogeneous bodies, heaped together without any method, but that each mineral species which is met with on our globe, is formed of elements definitely combined; and that a cabinet of minerals ought to constitute part of every chemical museum, as essentially, as soluble and other salts which were formerly considered as distinct from the mineral kingdom. This was easily proved, in reference to more simple minerals, whose elements were found to exist, combined in atomic proportions, both in artificial and natural salts. Thus, the atomic weights of sulphuric acid and lime being determined when entering into the composition of what were at first ascertained to be atomic compounds, it was but reasonable, on the occurrence of these bodies in a native state, to assign to their ultimate particles the same atomic weights. Accordingly, sulphate of lime has been found abundantly in a native state, in two states,—first, as Cal $Sl + 2$ Aq, and second, as Cal Sl. In both of these instances the atomic weights of the sulphuric acid and lime were precisely the same as in the more familiar salts, sulphate of soda, muriate of lime, &c. Having ascertained that this held good in regard to one or more minerals, chemists were induced to extend their researches over the field of nature. They gradually discovered that some bodies possess actions which they would have long looked for in vain, if they had neglected this delightful and varied field of investigation. They found that a mineral termed Table Spar, afforded, by the analysis of eight different specimens from different localities, always, the same quantities of silica and alumina,—about 51 parts of the former, and 45 of the latter. Another mineral, Picrosmine, gave by analysis, 56 parts silica and 36 magnesia. What, then, were the legitimate deductions to be drawn from these analyses? Was it not correct to say that the silica acted the part of acid to the lime and manganese, as did the sulphuric acid in the instances previously alluded to? Hence the formulæ for table spar and picrosmine, it has been inferred, are Cal S^2 and Mg S^2. The discovery that silica acted as an acid, in simple combinations, was sufficient to entitle chemists to conclude that this important body continued to preserve its power of action in more intricate compounds, where several bases presented themselves, upon which it might exercise its agency. If, in the case of the table spar, an atom of iron had been present, we should have had Cal. S $+$ FS; the formula would have been extended; the composition would have been somewhat more intricate; and, if we had a third atom of silica, as in tersilicate of lime, we might have had a third base united with the third atom of acid. And all this with as much propriety as there is in representing the composition of the more familiar salt, alum, by K Sl. $+ 3$ Al Sl. $+ 25$ Aq.

To those who have occupied themselves with the important study of the mineral kingdom, we know that these observations are quite superfluous; but they may properly be urged in answer to such as term the analysis of stones (as they sneeringly designate the labours of the analyst) an abuse of the atomic theory; and they are peculiarly applicable in turning our attention to the new work on mine-

ralogy and geology, whose title stands at the head of this article. The first volume consists of a description of 509 different species of minerals; the greater proportion of which have been subject to analysis, either by the author himself, or under his superintendence; and those, alone, can judge of the activity and enthusiasm with which, during the last ten years, these labours have been engaged in, who have been employed as fellow-workmen in the delightful, though arduous task. Before a properly arranged system can be formed, the elements of that system must be examined. Not only have the elements been scrutinized in the present instance, but they have been reduced into order, and of such a nature, as, we conceive, infinitely surpasses any which has been previously proposed. By the systems hitherto propagated the most dissimilar bodies have been associated. The classification of minerals, as of salts, should be simple, not complicated. The arrangement in the British Museum belongs to the latter class, and must be pronounced bad. The acids there distinguish the classes, and hence, the greatest confusion is produced; for the salts of each base constitute as many classes as the base forms combinations with acids. Thus lead is found in combination with at least seventeen different acids. These different minerals will, therefore, according to this arrangement, be deposited in seventeen different places. The base, however, of a salt, gives character, generally, to all the bodies into which that base enters as an element; the acid does not afford any such general character. If we class together the different sulphates, for example, we have bodies associated of all hues and dyes; but if we place the salts of copper in juxtaposition, the merest tyro would instantly discover the propriety of such an arrangement. This is the plan which has been adopted in the present work. We shall give a short view of the arrangement, in a tabular form, with the symbols:

CLASS I.—ACID BASES.

Genus.	Symbols.
I. Carbon.	C
Carbonic acid.	$\dot{C}$
II. Boron.	B
Boracic acid.	$\overline{B}$
III. Silicon.	
Silica.	S
IV. Phosphorus.	Ph
Phosphoric acid.	$\dot{Ph}$
V. Sulphur.	Sl
Sulphuric acid.	$\dot{Sl}$
VI. Selenium.	Sel
Selenic acid.	$\dot{Sel}$
VII. Tellurium.	Tl
VIII. Arsenic.	As
Arsenic acid.	$\dot{As}$
Arsenious acid.	$\overline{As}$

IX. Antimony.	St
Protoxide of antimony.	st
Deutoxide of ,,	$\overline{st}$
X. Chromium.	Ch
Oxide of chromium	ch
Chromic acid.	$\dot{Ch}$
XI. Molybdenum.	Ml
Molybdic acid	$\dot{Ml}$
XII. Tungsten.	Tn
Tungstic acid.	$\dot{Tn}$
XIII. Columbium.	Cl
Columbic acid.	$\dot{Cl}$
XIV. Titanium.	Tt
Titanic acid.	$\ddot{Tt}$
XV. Vanadium.	Vn
Vanadic acid.	$\dot{Vn}$

CLASS II.—ALKALINE BASES.

Genus.   Symbols.
I. Ammonia.   Am.
II. Potash.   K
III. Soda.   N
IV. Lithia.   L
V. Barytes.   Br
VI. Strontian.   Str
VII. Lime.   Cal
VIII. Magnesia.   Mg
IX. Alumina.   Al
  1. Pure, or combined with bases, 7 species.
  2. Simple salts, 24 species.
  3. Double anhydrous salts, 39 species.
  4. Do.   do.   soluble in water, 3 species.
  5. Do.   do   insoluble, 4 species.
  6. Double hydrous silicates, or zeolites, 39 species.
  7. Triple aluminous salts, 15 species.
  8. Quadruple salts.
X. Glucina.   G.
XI. Yttria.   Y
XII. Cerium.   Cr
    Protoxide of cerium. $\underline{cr}$
    Peroxide of   ,, $\overline{cr}$
XIII. Zirconia.   Zr
XIV. Thorina.   Th
XV. Iron.   F
    Protoxide of iron.   f
    Peroxide of ,, $\overline{f}$
  1. Uncombined, or united to a simple substance.
  2. a, Oxygen salts.
    b, Double oxygen salts.
    c, Triple oxygen salts.
  3. Sulphur salts.

XVI. Manganese.   Mn
    Protoxide of do.   mn
    Sesquioxide of do. $\overline{mn}$
    Binoxide of   do.   mn
  1. Combined with simple bodies
  2. Oxygen salts.
XVII. Nickel.   Nk
    Oxide of nickel.   nk
XVIII. Cobalt.   Cb
    Oxide of cobalt.   cb
XIX. Zinc.   Z
    Oxide of zinc.   zn
XX. Lead.   Pl
    Protoxide of lead. $\underline{pl}$
    Peroxide of lead. $\overline{pl}$
XXI. Tin.   Sta
    Oxide of tin.   sta
XXII. Bismuth.   Bs
    Oxide of bismuth.   bs
XXIII. Copper.   Cp
    Red oxide of copper. $\underline{cp}$
    Black oxide of copper. $\overline{cp}$
XXIV. Mercury.   H
XXV. Silver.   Ag
XXVI. Uranium.   Ur
    Protoxide of uranium.   ur
    Peroxide of   ,, $\overline{ur}$
XXVII. Palladium.   Pal

CLASS III.—NEUTRAL BASES
  1. Gold.   Au
  2. Platinum.   Plt
  3. Iridium.   I

Of the 509 Species (by far the most complete Mineral List hitherto presented to the world) described under these heads, there are above 50 Species entirely new, which have been first analysed in the laboratory at Glasgow.   These are:

	Name	Formula	Form	Sp. gr.
1	Kilpatrick quartz	$18\,S + Aq$	Four-sided pyramid	2·525
2	Arseniet of antimony	$As^2\,St$	Granular	6·130
3	Calcareo-sulphate of barytes	$Cal\,\ddot{S}l + 2\frac{1}{4}\,Br\,\ddot{S}l$	Foliated	4·190
4	Baryto calcite	$4\frac{1}{2}\,Cal\,\ddot{S}l + Br\,\ddot{S}l$; $Br.\,\ddot{S}l + 2\,Br\,\dot{C}$	Foliated	3·868
5	Sulphato carbonate of barytes	$7\,Str\,\ddot{S}l + 3\,Br\,\ddot{S}l$	Six-sided prisms	4·141
6	Baryto sulphate of strontian	$7\,Str\,\ddot{S}l + 2\,Cal\,\ddot{S}l$	Laminated	3·921
7	Calcareo sulphate of strontian	$4\,Cal\,\ddot{S}^2 + NS_3$	Plates	3·810
8	Wollastonite	$2\,Cal\,\dot{C} + Br\,\dot{C}$	Fibrous	2·850
9	Bicalcareo carbonate of barytes	$6\,Cal\,S + 5\,Al\,S$	Dodecahedrons	3·718
10	Xanthite		Oblique prism	3·22
11	Raphilite	$Cal\,S^3 + (\frac{2}{3}\,Al + \frac{1}{3}\,f)\,S^2 + (\frac{1}{3}\,K + \frac{1}{3}\,Mg)\,S^2$	Four-sided prisms	2·85
12	Polyadelphite	$7\,Cal\,S + 5\,f\,S + \frac{3}{8}\,\overline{Mg}\,S + 1\frac{1}{4}\,Al\,S + \underline{mn\,S}$	Grains	3·76
13	Retinalite	$3\,Mg\,S^2 + \frac{2}{5}\,\overline{NS} + 8\frac{1}{4}\,Aq$	Compact	2·49
14	Dysluite	$5\,f\,Al + 2\,Z\,Al + \underline{mn\,Al}$	Imperfect.	3·55
15	Sulphate of alumina	$Al\,\ddot{S}l + 6\,Aq$	Scales	1·66
16	Gilbertite	$7\,Al\,S + (Cal + Mg + f)\,\underline{S}^2 + 1\frac{1}{4}\,Aq$	Plates	2·64
17	Hydrous Bucholzite	$5\,Al\,S + Aq$	Granular	2·85
18	Tuesite	$3\,Al\,S + 2\,Al\,Si\frac{1}{4} + 3\,Aq$	Compact	2·55
19	Nacrite	$Al\,\ddot{S}^2$	Scales	2·78
20	Davidsonite	$Al\,\ddot{S}^2\frac{1}{4}$	Four-sided prisms	2·36
21	Quatersilicate of alumina	$Al\,\ddot{S}_4$	Ib.	2·68
22	Brown manganese garnet	$2\,Cal\,S + Al\,S + f\,S + \overline{mn\,S}$	Ib.	3·82
23	Pipestone	$2\,Al\,\ddot{S}_2 + (N + Cal + \overline{Mg})\,\ddot{S}_2 + Aq$	Compact	2·608
24	Soda alum	$3\,Al\,\ddot{S}l + N\,\ddot{S}l + 20\,Aq$	Fibrous	
25	Stellite	$4\,Cal\,S^2 + Mg\,\ddot{S}^2 + Al\,S + 2\frac{1}{2}\,Aq$	Four-sided prisms	2·612

					Sp. gr.
26	Zeuxite	$3 \, Al \, S + f^2 \, S + Aq$	.	Rectangular prisms	3·051
27	Plinthite	$3 \, Al \, S + 2(\frac{1}{3} f + \frac{1}{8} Cal) \, S + 6 \, Aq$	.	Compact	2·342
28	Antrimolite	$5 \, Al \, S + (\frac{3}{4} Cal + \frac{1}{4} K) \, S_3 + 5 \, Aq$	.	Fibrous	2·096
29	Glottalite	$Cal \, S + Al \, S1\frac{1}{4} + 2\frac{3}{4} \, Aq$	.	Four-sided prisms	2·181
30	Harringtonite	$3 \, Al \, S1\frac{1}{4} + (\frac{2}{5} Cal + \frac{1}{3} N) \, S + 2 \, Aq$	.	Compact	2·217
31	Lehuntite	$3 \, Al \, S^2 + (\frac{5}{8} N + \frac{1}{3} Cal) \, S^2 + 3 \, Aq$	.	Ib.	1·953
32	Cluthalite	$4 \, (Al + Mg) \, S^2 + (N+f) \, S^2 + 3 \, Aq$	.	Rectangular prisms	2·166
33	Erinite	$6 \, Al \, S^2 + f \, S^4 + 16 \, Aq$	.	Compact	2·04
34	Rhodalite	$3 \, Al \, S^4 + f \, S^4 + 16 \, Aq$	.	Rectangular prisms	2·00
35	Neurolite	$5 \, Al \, S_4 + (Cal + Mg) \, S^4 + 2\frac{1}{2} \, Aq$	.	Fibrous	2·476
36	Bytownite	$5 \, Al \, S + 3 \, (Cal + Mg) \, S^2 + 3(N+f) \, S^2$	.	Granular	2·801
37	Vermiculite	$2 \, Mg \, S^2 + Al \, S^2 + f \, S^2 + 3 \, Aq$	.	Plates	2·525
38	Kirwanite	$f \, S + Cal \, S + Al \, S_2 + Aq$	.	Fibrous	2·941
39	Scorilite	$3 \, Al \, S^2 + 3 \, Cal \, S^2 + f \, S^3$	.	Compact	1·708
40	Phyllite	$9 \, Al \, S + 3 f \, S + 3 \, Mg \, S + K S + 3\frac{1}{4} \, Aq$	.	Plates	2·889
41	Huronite	$22 \, Al \, S + 3\frac{1}{3} \, Cal \, S^2 + 1\frac{1}{3} f \, S_2 + Mg \, S_2 + 5\frac{1}{2} \, Aq$	.	Granular	2·862
42	Crucilite	f	.	Oblique prisms	3·579
43	Dihydrate of iron	$\ddot{f} + \frac{1}{2} \, Aq$	.	Right rhombic prism	4·876
44	Anhydrous silicate of iron	$f \, S$	.	Four-sided do.	3·884
45	Polylite	$\bar{9} \, (f + mn) \, S + 4 \, Al \, S + 3\frac{1}{3} \, Cal \, S^2$	.	Plates	3·231
46	Hydrous Sesquihinoxide of manganese	$3 \, mn + 2 \, mn + 5 \, Aq$	.		3·312
47	Newkirkite	$3 \, mn \, Aq + 2 \, mn \, f^2$	.	Rectangular prism	4·078
48	Silicate of manganese	$mn \, S$	.	Right oblique prisms	3·824
49	Sesqui-silicate of manganese	$8 \, \overline{mn} \, S1\frac{1}{4} + f \, S_3$	.	Double oblique do.	3·586
50	Ferruginous silicate of manganese	$3 \, mn \, S + f \, S1\frac{1}{4} + Aq$	.	Obtuse rhombohedron	3·034
51	Vanadiate of lead	$Pl \, \overline{Chl} + 9 \, pl \, \underline{Vn} 1\frac{1}{2}$	.	Six-sided prisms	6·663

Such is a general view of the contents of the first volume. It commences with an introduction explanatory of the nomenclature of the external characters of minerals, and exhibiting a view of the system of crystallography adopted by Mohs, for the purpose of enabling the English reader to consult Haidinger's admirable translation of Mohs' works. The volume concludes with three tables, in the first of which are given the specific gravity, hardness, and form of the crystals of minerals, in the order of the chemical arrangement. The second affords a list of minerals arranged according to the specific gravity, beginning with Scheererite the lightest; and the third supplies a list of minerals in the order of their hardness. Mineralogists will at once appreciate the utility of these tables.

The first 345 pages of the second volume are devoted to an outline of geology, and a valuable and complete table of the fossils, plants, and animals found in the mineral kingdom.

The first chapter, on the Temperature of the Earth, is full of most important matter. In order to determine the state of the question in reference to the existence of a central fire, the author has collected all the observations that have hitherto been published on the temperatures, from the surface of the earth to the greatest depth that has been attained by man. From these it appears that, taking the mean of nineteen observations, there is an increase of $1°$ F. for every 50 feet of descent. This is the evidence which many bring forward for the existence of a central fire. The author, however, shews, that according to the observations of Mr. Moyle, made during a series of years in Cornwall, the high temperature of these mines continues only while they are working. When they are abandoned they are soon filled with water, which remains stagnant, and the temperature gradually sinks, till it approaches that of the mean temperature of the place. 2. That the temperature of the earth is regulated entirely by the sun, for, the higher the sun is elevated above the horizon and the longer it continues above the horizon, the higher is the temperature. If the temperature increased $1°$ for every 50 feet, a descent of 12 miles, or a point by so much nearer the centre of the earth than the position of the equator, should afford a temperature, allowing for radiation, of $1200°$. Now, this ought to be the temperature of the poles, because they are 12 miles nearer the earth's centre than the equator. Their temperature is, however, $— 13°$, and hence, this seems a fatal argument to the notion of a central fire. But, although the idea of a *central* fire is not supported by the facts with which we are acquainted, it is not unlikely that an *internal* fire exists, which gives origin to those vast volcanic regions and earthquakes which are continually altering the aspect of the earth's surface. If we were to consider this fire as approaching nearer the surface in some places than in others, we might have, perhaps, an explanation of the relative causes of volcanoes and earthquakes.

The remainder of the geological portion is divided according to the formations, beginning at the surface. Many original observations are detailed, especially in reference to the geology of Scotland, where the occurrence of most remarkable alterations in the relative levels of the sea and land is minutely detailed. The Glasgow coal beds

are accurately described. The annual consumption of coal in the Glasgow markets, it is stated, amounts to 870,000 tons. But one of the most curious facts detailed, is the discovery, by the author, of a bed of coal in basalt, near Dalry, in Ayrshire.

This bed is 4 feet thick, and is situated some hundred feet below the summit of Beadlanhill, which is elevated 903 feet above the sea. Its specific gravity is 1·317. Colour brown; it is very hard. Burns with a lively flame, and leaves 25·77 per cent. of earthy matter. It contains vegetable impressions differing from any that have hitherto been described, as derived from the coal formation. They appear to be *fucoides*. The only other locality, where it is believed, coal has been found in basalt, is at Fairhead, in Ireland, but no fossils have been observed in it.

The latter part of the second volume, consisting of above 200 pages is devoted to rules for the analysis of mineral substances, including stony minerals, metallic alloys, and mineral salts. This portion of the work is worth the attention of geologists as well as mineralogists, as it must be obvious to every one, who casts his eye over the vague speculations of too many of our present geologists, that without the application of chemistry, mineralogy, and natural history, geology is but a name.

II.—*The Agricultural and Horticultural Annual for* 1836, *&c.* Simpkin and Marshall, London; Baxter and Son, Lewis, 8vo.

THE object in projecting this publication was " to record the experience of the most practical men in their researches and experiments, and to collect what was valuable and new." Accordingly we find, that this object has been completely fulfilled. An excellent selection of articles has been made from various sources, which affords a most gratifying view of the recent labours, of our farmers, gardeners, and botanists. We find besides, several useful original communications. A plan for securing corn ricks from mice, detailed by Mr. Jenner cannot fail to be acceptable to agriculturists. The rick should be built nearly perpendicularly and cut round about 2 feet high from the ground, slanting from the top to the bottom about 18 inches. The part that is cut is plastered over with mortar made of clay, and the whole white-washed. This plan is practised in Norfolk, and proves completely successful.

In an excellent paper taken from the Gardener's Magazine, we learn, that from 1801 to 1810, 94 trees and shrubs were introduced into this country; from 1811 to 1820, 374 were introduced; and from 1821 to 1830, 318. If we compare former centuries with the present, the activity displayed during the short period which has elapsed of the latter, appears quite astonishing. The number of trees and shrubs introduced in the 16th century was only 89, 17th 131, 18th 445, and in the first 3 decades of the 19th 699 ! The total number of foreign trees and shrubs introduced into this country may be about 1400 up to the present time.

John Fraser a native of Invernesshire, and John Lyon a son of Mr. Lyon of Gillogie in Forfarshire, were two very active collectors. But there is none who has contributed more to the decoration of our

gardens, and the prospective beauty of our forests than worthy David
Douglas. · The number of herbaceous plants which he introduced
amounts to 100, and of trees and shrubs to 50.   He was born at
Scone, near Perth, and served his apprenticeship as a gardener in the
gardens of the Earl of Mansfield.   In 1817 he removed to Sir Robert
Preston's garden, at Valleyfield, and shortly afterwards went to the
Glasgow Botanic Garden.   Here he attracted the attention of Dr.
Hooker, whom he assisted in obtaining materials for the Flora Sco-
tica.   In 1823, on the recommendation of Dr. Hooker, he was dis-
patched by the Horticultural Society to the United States, where
he greatly increased the Society's fruit trees.   He returned the same
year.   In 1824, an opportunity offering through the Hudson's Bay
Company of exploring the country adjoining the Columbia and Cali-
fornia, he sailed in July.   He touched at Rio de Janeiro, and dis-
covered the *Gesneria Douglassii*.   On Christmas day he. reached
Juan Fernandez, which he describes as "an enchanting spot, very
fertile, and delightfully wooded."   He arrived at Fort Vancouver,
on the Columbia, 7th April, 1825.   Here he made a great collection
of seeds for the Society, along with dried specimens which now form
part of the herbarium at Chiswick.   The *Pinus Lambertiana*, a
native of this part of the world, was one of his finest discoveries.   One
specimen measured 215 feet in length, and 57 feet 9 inches in cir-
cumference at three feet from the ground.   The cones were 16 inches
long and 11 in circumference.   The kernel of the seed is sweet and
is eaten by the Indians.   The rosin which exudes from the trees
when they are partly burned, loses its usual flavour and becomes
sweet, and is used by the natives as sugar.   The *Abies Douglassii* is
nearly the same size.   In 1827, Mr. Douglas passed from Fort Van-
couver across the Rocky Mountains to Hudson's Bay, where he met
Captain Franklin's party returning from their second expedition.
He returned with them to England, where he was elected free of
expense a Fellow of the Linnean, Geological and Zoological Socie-
ties, to each of which he contributed papers which display much
acuteness.   Extracts from his letters were printed in Brewster's
Journal, and a new class of plants, named in honour of him by Dr.
Lindley, was described in Brande's Journal.   After remaining in
London for two years, he again sailed for Columbia in 1829, where
he remained for some time adding to his former discoveries.   His re-
turn to England was expected by the very ship which conveyed the
intelligence of his horrible death, an event which was occasioned by
his falling into a pit made by the natives of the Sandwich Islands for
catching wild bulls, one of the latter being in it at the time.
    The Annual concludes with a Calendar and Almanack.
    Our readers will recollect that we drew their attention formerly
(vol. i. 159) to the important improvement introduced into the arts
by Mr. George Baxter, viz., that of printing with colours from wood ;
and that we then augured favourably of his success, and of the pro-
spect which we anticipated of his improving upon the process.   We
are happy to state, that his success has been most flattering.   The
frontispiece to the present volume, which is printed from a plate, and
therefore, exhibits another improvement for which he has taken out
a patent, delineates a South Down sheep true to the life.   It requires

only to be seen to be appreciated, and proves the advantageous nature of this new discovery, (as it may be justly denominated), to the purposes of the naturalist.

III.—*Supplement to Captain Sir John Ross's Narrative of a second Voyage in the Victory, in search of a North-West Passage, containing the suppressed facts, &c.* By JOHN BRAITHWAITE.

THE author of this pamphlet is the engineer who constructed the machinery of the steam-vessel, which Captain Ross employed in his expedition to the Polar Seas. The object of the publication is to defend the author's character, as a mechanic, against the accusations of Sir John Ross. According to the statements of the latter, the machinery failed in the object for which it was intended; it is termed *execrable*, and the engineer is charged with *gross negligence*. Mr. Braithwaite denies that these epithets are applicable, and affirms, 1. That Capt. Ross deceived him as to the real object of the machinery, having positively ordered the engines to be placed *under the water line*, to be out of the reach of shot. He stated, that he wished to try the experiment of condensing the steam in tubes, and to use the same water over and over again; for which purpose a condensing apparatus was made *(never before tried)*. 2. The patent steam boiler of the author, and his co-patentee Captain Ericsson, was ordered to be supplied, which though it promised well, had never been used for any *practical purpose*. 3. Captain Ross refused to acquaint the engineers with the nature of the paddles he was going to use, and thus concealed from them a material circumstance to be taken into account in proportioning the size of the cylinders, for which the only instruction given was, that the engines should make from 35 to 40 strokes per minute. 4. Without being consulted whether the introduction of cog wheels was advisable or not, the engineers received orders to make such wheels for communicating the power of the engines to the paddle-wheels. They were not consulted upon the proper weight of the paddle-wheels, &c. The consequence was, that Captain Ross immersed the paddle-wheels nearly to their axes; the speed of the vessel was, therefore, impeded in a great degree.

That these causes are sufficient to account for the failure in question, must at once be obvious to every person. In what way Captain Ross explains away the errors, of which Mr. Braithwaite accuses him, we are ignorant, as the extravagant price of his work, has made it a sealed book, not only to us, but to all those who are most interested in such subjects.

IV.—*Tabulæ Anemologicæ, or Tables of the Winds, exhibiting a new method of registering the direction of the Wind, &c.* By W. R. BIRT. London, &c.

THE plan of registering the wind developed in this publication, depends on certain periods, during which, the wind is observed to blow

from particular quarters. To these periods the author applies the term *anemonal*, or periods belonging to the wind. They form the divisions of a map, upon which the curves of the wind are exhibited. The proportion of rain is also well shown by an ingenious method. The author purposes publishing on the 1st of January, an *anemonal* table, exhibiting the aerial currents, at Carlisle, Liverpool and London, and we believe also, at Abbey St. Bathans, (from the observations of our valuable correspondent Mr. Wallace), during January and February, 1835, and a continuation of the present table on the 1st of February. We must refer our readers to the work itself, which is very moderate in price (4d.) because it would be impossible to give an intelligible description without a plate, and because it appears to promise very valuable assistance to meteorologists.

ARTICLE IX.

SCIENTIFIC INTELLIGENCE.

I.—*Proceedings of the Ashmolean Society, of Oxford.*

June 26, 1835.—The following query was proposed by a member :
In what way can we satisfactorily explain the mode in which spiders carry their threads from one object to another at considerable distances through the air ?

Dr. Daubeny exhibited a specimen of the bromelia pinguis, a native of the West Indies, which flowered this autumn in the open air in the garden of Mr. Shirley of Eatington Park, near Shipston-upon-Stour. This plant has rarely blossomed in Europe even under glass, although a drawing of it in flower is given in the Hortus Elthamensis; and the individual plant alluded to had been tried first in the pinery, and afterwards in the greenhouse, but had never put forth flowers, till it was taken out of doors, when it flowered, though the petals, never properly expanded.

A communication was also read by him respecting an electrical phenomenon stated to have occurred in the garden of the Duke of Buckingham at Stowe.

The following was the statement drawn up by his Grace's direction, of the circumstance alluded to.

" On the evening of Friday the 4th of September, 1835, during a storm of thunder and lightning, accompanied by heavy rain, the flower called *œnothera macrocarpa*, a bed of which is in the garden immediately opposite the windows of the manuscript library at Stowe, were observed to be brilliantly illuminated by phosphoric light.

" During the intervals of the flashes of lightning, the night was exceedingly dark, and nothing else could be distinguished in the gloom except the bright light upon the leaves of these flowers.

" Stowe, September, 23rd, 1835."

A paper was read by Prof. Rigaud on Halley's Astronomiæ Cometicæ Synopsis.

Halley had begun his calculations of cometary orbits in 1695, and appears to have completed them in 1702; but it was not till 1705

that he published his Astronomiæ Cometicæ Synopsis in the Philosophical Transactions for 1705. In this he gives the parabolic elements of 24 comets observed between 1337 and 1698, with the table which he formed for calculating their motions. This he reprinted separately at Oxford in the following summer; and an English translation was published the same year, which probably was his own, as he adopted it in the second volume of the Miscellanea Curiosa. The Synopsis was intended for the introduction to a larger work, and he printed it to secure his calculations from being lost, in case of any accident befalling him. The first edition contains a notice of some similarity (on which however he did not much depend) between the comets of 1661 and 1532, whose possible return in 129 years has not been verified. In 1715 the work was re-printed at the end of an English translation of Gregory's Astronomy. In this he first speaks of calculating the elliptical orbits, and brings forward the possible identity of the comets of 1105 and 1680. In 1719, with his volume of Astronomical Tables, he printed a new edition of the Synopsis, in which he entirely omits the mention of the comets of 1661, but gives elliptical elements for those of 1680 and 1682, and a comparison of the places calculated from them, with the observations which he could find on record. He had likewise discovered some earlier observations of the last, which agreed well with its revolving in an orbit of about 75½ years; and having pointed out the circumstances which retarded its return, he confidently concluded that it might be expected again in the latter end of 1758 or 1759.

Mr. Kynston exhibited, and presented to the Society, a preserved specimen of a grasshopper, to which were attached a number of species of worm, very long, slender, and convoluted, which had fixed themselves upon it, and destroyed it. It was found in Switzerland.

The President shewed a portion of wasp's nest made in a hollow in a sugar-loaf, into which the wasps had eaten, and composed of the blue and white paper in which the loaf was wrapped. The nest was discovered in the month of August, and appeared to have been begun not long before. No instance being as yet known of wasps going out from a nest already formed to construct another in the same year, it is most probable that the present nest was begun by a female wasp, which had survived the last winter, and not by any of the other wasps which were engaged in eating the sugar.

Dr. Daubeny stated, that during the last autumn he had made the discovery of fresh springs which evolve nitrogen gas.

The first of these was the tepid spring of Mallow in the county of Cork, a water which contains but very little solid matter. The gas evolved consisted of

Nitrogen 93·5. Oxygen 6·5.

It appears to issue from carboniferous limestone, the beds of which in its immediate neighbourhood are vertically disposed, intimating that they have been affected by some violent action since they were originally deposited.

The other spring, disengaging nitrogen, which he observed, was near Clonmell. It was a very clear but perfectly cold water, called St. Patrick's well, held in much veneration in the neighbourhood,

and resorted to by pilgrims in great numbers. Bubbles of gas rise up through it, which Dr. Daubeny found to consist of

Nitrogen 94. Oxygen 6.

The spring gushes out of the same limestone stratum, as that of Mallow.

November 20th. A notice was communicated from Mr. Kirtland respecting the worm exhibited at the last meeting by Mr. Kynaston, which had apparently destroyed a grasshopper. It is found to be the *gordius aquaticus,* or hair worm, so called from various contortions and knots into which it twists itself. In a communication made to Loudon's Magazine, vol. ii. p. 211, it is said to be often met with on the surface of garden or other ground in wet weather, as it is in water or clay, its common habitation.

The *gordius aquaticus* is not unfrequently found to inhabit the intestines of insects. *De Geer* (marshall of the court of the queen of Sweden, and member of the Academy of Stockholm, and who published a work intitled "Memoires pour servir à l'Histoire des Insectes" in 7 vol. 4to. 1752—1779) mentions these worms being found *in grasshoppers.* Dr. Matthey likewise mentions one of these worms being found in the body of a *grasshopper,* which was no less than 2½ feet in length.

Mr. Paxton mentioned a similar case in the instance of an earwig.

Mr. Johnson of Queen's, read a short account of some mathematical researches he had lately pursued on optical images. He was led to this remarkable result, that, according to the mathematical theory, the image of a straight line placed vertically in water, and also horizontally, are each the loci of equations of high dimensions and great complexity, and should be curves of high orders, but to the eye they are straight lines ; a very accurate construction of the curves, however, shewed that certain portions of them (which properly represent the image) will approach so near to straight lines as to be such to the eye. Drawings of these curves were exhibited.

Mr. Powell gave a communication on the dispersion of light, in continuation of former papers, in which he illustrated the subject by diagrams of the several spectra formed by prisms of water, oil of turpentine, flint glass, oil of cassia, oil of aniseed, and sulphuret of carbon, shewing their comparative refractive and dispersive powers.

Dr. Buckland read a further statement relative to the luminous appearance on the flowers of the œnothera, mentioned at the last meeting. It was distinctly stated that the luminous appearance continued uninteruptedly for a considerable length of time : it did not appear to resemble any electric effect : and the opinion which seemed most probable was, that the plant, like many known instances, has a power of absorbing light, and giving it out under peculiar circumstances.

Dr. Daubeny exhibited some specimens of sand and clay found in the bottom of the caverns, in limestone, at Michell's town, near Cork. The sand covered the bottom of the cave to an unknown depth, and was itself covered with a crust of stalagmite. The sand must have been washed in through a very narrow entrance ; and there is no

existing stream capable of so introducing it. No bones or other remains were found in it.

Dr. Buckland also explained the occurrence of such sand, &c. by diluvial action, and proceeded to remark a curious circumstance connected with these caverns. There has never been an instance in which any deposits have taken place at the bottoms of caves, except such as are composed of recent remains, and the mud, sand, &c. of the surface; debris and fossil remains of older formations never occur in them. The only instance known of any older remains in caverns, is that of the caves at Palermo, belonging to the later tertiary period, and containing shells, &c. of that formation perforated by pholades, though now raised 300 feet above the sea.

Dr. Buckland also observed that the origin of caves in limestone had during many years occupied his attention, and has always been considered by him one of the most difficult problems in geology. To a certain degree they have in many cases been the effects of mechanical violence producing lateral movements, and tearing asunder portions of solid rocks, during the elevation, or subsidence, of the strata in which they occur. In cases of this kind, the fractures are usually rectilinear, and partake of the nature of a slip or fault, never filled up. But the lateral enlargements and tubular communications that proceed in various directions from the main apertures, and the vaulted and dome-shaped expansions that occur at irregular intervals along the minor winding passages, cannot be referred to mechanical violence; and an adequate cause of their origin may possibly be found in the influence of acid vapours, (probably carbonic acid,) rising through fractures adjacent to these corroded portions of the limestone.

Caverns in solid limestone could not have been produced, like cells and cavities of various size in beds of porous lava, by air included in the viscid substance of the strata, before or during the progress of consolidation, because they are most abundant in limestones of the most compact character, and in which no other trace of cellular structure is to be found. Moreover, the interior of caverns usually presents an irregular carious surface, similar to that which is produced on a mass of limestone submitted to the action of an acid.

If these supposed acids were mixed with water, the lime thus dissolved would have been removed in a state of solution, and the sides of the caves would be found studded with the less soluble contents of the strata, such as siliceous concretions, and fragments of organic remains, standing in relief, as we often see them around the interior of these carious vaultings.

The organic remains in these strata, particularly the corals, are often disposed in such a manner as to shew that considerable time elapsed during the deposition of the successive beds of limestone in which they are enveloped; no accumulations of gas in connected cavernous expansions passing from one stratum into another could have taken place in beds of limestone thus deposited at successive intervals.

Dr. Daubeny expressed a doubt as to whether all caverns could be accounted for by aqueous corrosion alone, and conceived that the large vaulted chambers into which many of them suddenly expand,

may have been originally produced by an evolution of gaseous matter, whilst the rock itself was in a softened condition.

II.— *Gastric Juice.*

THE experiments of Dr. Prout, and of Tiedemann and Gmelin in reference to the gastric juice, are confirmed by those of Braconnot, and prove that there is no peculiar substance to which this appellation should be applied, but that the remarkable peculiarity of the stomach is the property which it possesses of secreting a great quantity of muriatic acid. The gastric juice examined by Braconnot was obtained from a dog. He found it to contain

1. Free muriatic acid in great abundance. 2. Muriate of ammonia. 3. Chloride of sodium in very great quantity. 4. Chloride of calcium. 5. A trace of chloride of potassium. 6. Chloride of Iron. 7. Chloride of magnesium. 8. Colourless oil with an acid taste. 9. Animal matter soluble in water and alcohol, in very considerable quantity. 10. Animal matter soluble in weak acids. 11. Animal matter soluble in water, and insoluble in alcohol (salivary matter of Gmelin). 12. Mucus. 13. Phosphate of lime. He found no trace of lactic acid.—(*Annales de Chimie*, lix. 348.)

III.—*Benzoyle, Benzimide, and Benzoine.*

IN distilling the essence of bitter almonds with well water, Laugier obtained a resinous substance which Laurent found to consist of 1. An oil containing the essence of bitter almonds; 2, benzoine; and 3, a crystalline body which he terms benzimide. Boiling alcohol dissolves the oil and benzoine, and on cooling benzimide falls. After filtration, by evaporation, the benzoine crystallizes and the oil remains in solution. The benzimide and residue are dissolved in boiling alcohol, and on cooling minute needles of benzimide separate.

Benzimide is white and destitute of smell, insoluble, very little soluble in boiling alcohol and ether. When heated, it burns with a red flame, leaving a brown residue. Nitric and muriatic acids dissolve it readily. Sulphuric acid dissolves it and acquires an indigo colour. When treated with pieces of potash and some drops of alcohol, benzoate of potash is formed.

It consists of carbon 74·86; hydrogen 4·94; oxygen 13·20; azote 7. This composition Laurent considers equivalent to bibenzoate of ammonia, with a deficiency of 4 atoms of water, or we may call it $C^{14} H^{5\frac{1}{2}} O N_{\frac{1}{4}}$. The benzamide of Wöhler and Liebig corresponds with the neutral benzoate of ammonia.

Benzoine was previously obtained from the essence of bitter almonds from which it may easily be extracted by means of potash. It consists of carbon 78·652; hydrogen 5·772; oxygen 15·577. This corresponds with $C^{14} H^6 O$, and is isomeric with hydret of benzoyle.

Benzoyle was formed by passing chlorine over fused benzoine. The product was dissolved in alcohol, and crystallized. Benzoine is

yellowish, insipid, insoluble in water, soluble in alcohol and ether. Crystals six-sided prisms, terminated by summits with three pentagonal faces. They burn with a red flame. Hot sulphuric acid dissolves them, and water precipitates them from the solution. Potash when dissolved in water does not alter them, but when an alcoholic solution is employed, a fine colour of turnsol is produced. If this solution is evaporated, a salt is obtained which forms with sulphuric acid a beautiful pink solution. Benzoyle consists of carbon 80·43; hydrogen 4·91; oxygen 15. This Laurent considers is represented by $C^{14} H^5 O$. Hence, we see that the chlorine has removed an atom of hydrogen.—(*Ann. de Chim.* lix. 397).

IV.—*Deaths of Signior Nobili and Dr. Stromeyer.*

WE regret to announce the death of M. le Chevalier Leopold Nobili [de Reggio, which happened at Florence on the 5th of August last, from an affection of the chest. Nobili, in the earlier part of his life, served in the army. At what age he began to turn his attention to philosophy does not appear, but after being occupied with some theoretical speculations upon magnetism and light, he directed his energies in 1825 to experimental researches; he invented the galvanometer with two needles, and subsequently added to this first invention that of the comparative galvanometer. 2. The discovery which introduced Nobili to the scientific world was that relative to the colours developed upon metallic plates, which serve as poles in different solutions for electro-chemical decompositions. 3. He invented the thermo-multiplier; for although Melloni assisted him, yet the idea originated with Nobili. 4. He published some papers relating to electro-physiological phenomena. 5. He made researches on the production of electricity by heat, and on chemical action, and on the relations which subsist between the two modes in which electricity is developed. 6. He directed his attention to the study of magnetism, and more particularly to the production of electric currents by the influence of magnets. His papers were published in the *Bibliotheque Universelle*, vols. 29, 33, 34, 35, 36, 37, 40, 44, 45, 47, 56, 57, 64.

Dr. Frederick Stromeyer, Counsellor and Professor of Chemistry and Pharmacy in the University of Gottingen, and Inspector general of the Apothecaries for the kingdom of Hanover, died on the 18th of August last. He was originally a botanist, and only turned his attention to chemistry when he obtained the chemical chair at Gottingen. He then went to Paris and studied chemistry in Vauquelin's laboratory. In 1817, he discovered the metal cadmium, and in 1821 he published an admirable volume of mineral analyses under the title of *Untersuchungen über die mischung der Mineralkörper und anderer damit verwandten Substanzen*, containing 30 analyses.

By the death of Stromeyer the University of Gottingen has experienced a heavy loss, and a blank has been created among the supporters of Science which will not soon be supplied.

Meteorological Journal,

Kept at the Manse of the Parish of Abbey St. Bathan's, Berwickshire, Lat. 55° 52′ N. Long. 2° 23′ W. at the height of about 450 feet above the sea. By the Rev. John Wallace.

NOVEMBER.

DATE. 1835.	THERMOMETER. IX. A.M.	X. A.M.	III. P.M.	X. P.M.	HYGROMETER (Leslie's.) IX. A.M.	X. A.M.	III. P.M.	X. P.M.	BAROMETER. At 32° of Fahrenheit. IX. A.M.	X. A.M.	III. P.M.	X. P.M.	Rain in Inches. Weekly.	Direction of Wind at X. A.M.	REMARKS.
Su. 1	40	43	43¼	29½	8	14	11	3	29·664	29·670	29·704	29·751		N.E.	Very calm, thin haze, occasional masses of hazy clouds, deposition in the evg.
Mo. 2	33½	36	39¼	40¼	9	12	6	3	29·693	29·693	29·634	29·600		E. by S.	Very calm, hoar frost in the morning, with dense fog, hazy during the day.
Tu. 3	43½	44	41½	41½	7	9	4	3	29·569	29·569	29·538	29·553		E. by S.	Strong wind, often boisterous, sky overcast, frequent heavy showers.
We. 4	40½	40½	41½	38¼	7	9	12	11	29·557	29·558	29·518	29·518	0·937	S.E. by E.	Brisk wind, sky overspread with hazy clouds, large masses floating below.
Full M. Th. 5	37	38½	35½	35	13	15	5	5	29·504	29·504	29·476	29·431		S.E. by E.	Calm, sky overcast, P.M. occasional snow showers.
Fr. 6	35½	36¼	37	37	1	3	1	2	29·374	29·374	29·397	29·470		S.E. by E.	Gentle wind, sky overcast, showery, evening cloudy.
Sa. 7	32½	32½	35	33	4	0	7	0	29·428	29·417	29·364	29·369		S.E.	Brisk wind, continued snow, evening partially clear.
Su. 8	37½	39½	41	40	4	7	7	2	29·492	29·497	29·538	29·668		N.W.	Calm, A.M. clear, P.M. hazy clouds prevalent, evening rainy.
Mo. 9	38½	39½	37½	35½	0	14	5	2	29·830	29·834	29·846	29·928		E.	Calm, A.M. cloudy, P.M. frequent showers, evening partially clear.
Tu. 10	40½	40¼	40¼	34½	11	12	14	2	30·034	30·040	30·040	30·055		E.S.E.	Calm, heavy clouds rapidly formed and dissolving in showers.
We. 11	36½	36¼	37½	35	12	12	16	11	30·005	29·996	29·925	29·893	1·375	W.N.W.	A.M. brisk wind, sky cloudy and often overcast, evening calm, heavy clouds.
Th. 12	40½	41¼	41	40	7	7	5	5	29·931	29·942	29·954	30·000		NWbyW	Calm, sky lowering, frequent heavy showers.
Fr. 13	37½	37¼	36¼	36	15	14	12	2	30·016	30·011	29·944	29·839		N.	Breeze very gentle, sky for the most part cloudy, wind rising in the evening.
Sa. 14	40¼	40	41	41	9	10	9	5	29·726	29·729	29·679	29·764		W.N.W.	Brisk wind, cloudy sky, showery.
Su. 15	39½	39¾	38	36¼	9	10	10	10	29·795	29·777	29·706	29·597		W.N.W.	Calm, sky often obscured with heavy clouds, evening clear.
Mo. 16	41½	42	41¼	45¾	11	11	7	8	29·467	29·477	29·395	29·280		W.	A.M. calm, and clear, P.M. brisk wind, evening a fine mackerel sky.
Tu. 17	47½	48¼	49½	47	10	11	0	11	29·216	29·209	29·153	29·152		W.N.W.	Strong gusty wind, cirrostratus and cirrocumulus, evg. calm, auroró borealis.
We. 18	51¼	51¼	44¼	42½	10	15	22	9	28·766	28·710	28·893	29·131	1·063	W.N.W.	Boisterous wind, with hazy clouds, evening calm and clear.
Th. 19	37½	39	42	42	8	8	6	6	29·350	29·350	29·321	29·211		W.N.W.	Calm, A.M. cirrostratus prevalent, P.M. overcast, evening rain, rising wind.
New M. Fr. 20	46½	46½	47	47½	6	7	8	7	29·109	29·115	29·066	28·902		SW.byW	Strong wind, A.M. light rain, P.M. overcast, evening rain.
Sa. 21	47½	46¼	43½	49¼	19	18	4	4	29·012	28·981	28·951	28·826		W. by N.	Strong wind, A.M. cloudy, P.M. heavy rain.
Su. 22	44	46½	43½	38	11	15	8	5	29·010	29·023	28·986	29·102		SW.byW	Calm, A.M. cloudy, P.M. rain, evening clear with deposition.
Mo. 23	34½	36½	40½	43½	4	4	0	0	29·204	29·190	29·151	29·173		SE.by E.	Calm, A.M. foggy, P.M. rain, evening overcast but fair.
Tu. 24	44½	44	46	46	12	9	10	9	29·144	29·127	29·063	29·000		SE.by E.	Sky overspread with hazy clouds of the cirrostratus formation, calm interrupted
We. 25	48½	50¼	49½	49½	4	7	4	5	29·146	29·165	29·088	28·945	0·788	SE.by E.	Same general character as yesterday; evening rainy. [by loud gusts of wind.
Th. 26	47	47	47¼	47½	4	5	2	0	29·070	29·045	28·913	28·630		SE.by E.	Gentle breeze, P.M. partially clear, P.M. hazy fog, evening calm and clear.
Fr. 27	49½	49	41¾	41½	14	14	19	4	28·556	28·563	28·622	28·648		SW.byW	Brisk wind, sky overspread with cirri and cirrostratus, evening calm and clear.
Sa. 28	38	39½	41	34½	3	5	4	2	28·697	28·717	28·809	28·917		SW.byW	Calm, cirri and cirrostratus, P.M. some rain, hazy clouds, evening deposition.
Su. 29	35½	36¼	37½	43	2	2	2	0	28·921	28·921	28·864	28·679	0·325	SW.byW	Calm, cirrostratus tending to cymoid formation, evening, rain with rising wind.
Mo. 30	41½	41½	45	44½	2	2	0	0	28·666	28·666	28·570	28·549		SW.byW	Calm, A.M. cloudy, P.M. thick fog with heavy rain.
Means.	40·8	41·7	41·8	40·3	8	9	8	5	29·365	29·362	29·337	29·319	4·488		

Means { Therm. 41°. Hygr. 7·0 for the { Bar. 29·340 in. Month } — Mean temperature of spring water 46°¾ Mean point of deposition 36°·9 Moisture in a cubic inch of air ·001575 grs.

RECORDS

OF

GENERAL SCIENCE.

ARTICLE I.

Memoir of John Napier, Baron of Merchiston.
By J. B. Biot.*

Montaigne, in his Chapter of Proper Names, has asked, To whom belongs the honour of so many victories—to Guesquin, Glesquin, or Geaquin, since the name of this distinguished person has assumed all these different forms? If intellectual conquests and military glory can admit of any analogy, and we leave this to be decided, we might ask the same question in reference to the subject of our Memoir, whose simple mathematical invention has, as it were, lengthened a hundred fold, the scientific lives of Kepler, Halley, Bradley, Mayer, Lacaille, Piazzi, Delambre; has extended that of Laplace, and even that of Newton; and continues, indefinitely, a similar prodigy to those whose zeal, if not genius, is applied, after these great men, to the mathematical study of natural phenomena. For we are still ignorant whether the discovery of Logarithms was due to Neper, Napeir, or Napier.† Even at the time when it was published in 1614, the author was so little known beyond the limits of his own country, that Kepler, who received and employed this invention with

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* Journal des Savants, March, 1835. (This constitutes a critique, by Biot, on the Life of Lord Napier, from the pen of Mark Napier, Esq. We publish it for the sake of " audi alteram partem."—EDIT.)

† In a letter to his father his signature is *Neper*; at his dedication of the Explanation of the Apocalypse to King James VI. it is *Napeir*; at his will it is *Naipper*. It is commonly spelled *Napier*.

enthusiasm, as a wonderful assistance in the composition of his Rudolphine Tables, was unacquainted with it till 1617; and even then, possessed but an imperfect knowledge of it, having only seen Napier's book at Prague, without being able to study it; so that, unfortunately for him, he did not appreciate it, as may be learned from a remarkable passage in a letter written by Kepler to his friend Schikkart, dated 11th March, 1618. The passage is, " Extitit Scotus Baro, cujus nomen mihi excidit qui præclari quid præstitit, necessitate omni multiplicationum et divisionum in meras additiones et substractiones commutatâ; nec sinibus utitur. Attamen opus est ipsi tangentium canone; et varietas, crebritas difficultasque additionum, substractionumque alicubi laborem multiplicandi et dividendi superat" (Epist. ad Keplerum Lipsiae 1718, in fol. p. 672). The last part of this passage shews us that Kepler, at first sight, had formed an erroneous opinion of Napier's method. The objection, *attamen opus est ipsi tangentium canone*, requires some explanation. In the original publication of his discovery, in 1614, of which I have examined a copy, from the library of Walknaer, Napier does not give a special table for logarithms of natural numbers, but only for sines, co-sines, and tangents of arcs. Thus, when it is necessary to find the logarithm of a given number, he supposes that it may be considered as a natural sine if it is included between 0 & 1, and as a natural tangent if it exceeds these limits. In the first case, the logarithm sought is found immediately among those of the sines which the table of Neper gives; in the second case, it is necessary to find first in a table of natural tangents the arc which corresponds to the given number, and with this arc the table of Neper gives the logarithm.

Having soon after, however, met accidentally with a short and clear exposition of this discovery, " I learned" said he, " its nature, and scarcely had I tried an example of the process, when I found, to my great delight, that it far surpassed all the attempts at abbreviation which I had attempted for a long time past." He set one of his pupils to the work immediately, and made him calculate logarithmic tables after the method of Napier; employed them with success in completing the calculation of his Rudolphine Tables, which had hitherto cost him immense labour; and even

changing the whole plan of these tables, he boldly gave
them a new form, which fitted them for the application of
the logarithms. Upon what accidents does not the progress
of human intelligence depend! The Rudolphine Tables
appeared in 1627, six years, only, before the death of Kepler.
Who knows if without the use of the logarithms he could
have finished them? They must become the fundamental
basis of all our ulterior knowledge in reference to the system
of the world; for being established for each planet on the
conditions of elliptical movement; and for the relations of
the orbits between them, on the proportion of the square
of the times of revolution to the cubes of the semi-great
axes, their constant agreement with the heavens presents
a constant epitome, as well as proof, of the great astrono-
mical laws, justly termed the laws of Kepler, from which
Newton has mechanically deduced that of the central force,
proportional to the masses, and reciprocal to the square of
the distance. But, if the general conditions of the planetary
motions had not been known and proved, Newton would not
have been able to ascend to the law of the force; so that,
without the invention of logarithms, which in some measure
enabled Kepler to live long enough to finish his task, univer-
sal gravitation might have, perhaps, still remained undis-
covered. This fortunate revolution in the tables and cal-
culations of Kepler has been described and celebrated by
Kepler himself, in a letter to Napier, dated 28th July, 1619,
which he placed before his Ephemerides for 1620. This
important document for literary history has become so rare,
that neither Montucla nor Delambre knew of its existence.
Fortunately, the Bodleian Library of Oxford possesses a
copy, of which, Napier's biographer, (Mark Napier, Esq.)
has presented an exact copy to the public. It is from this
letter that the preceding details are taken. Napier never
received this letter which would have filled him with joy.
He died two years before, on the 4th April, 1617; but
Kepler was ignorant of the fact. So difficult and slow
was scientific communication in these times of war and
storms, caused by the influence of political interests and
change in religion!

If continental Europe was in this condition, the state of
Scotland was still worse. The inhabitants of its Highlands,

divided into half-barbarous tribes, lived in a succession of
wars, and perpetual robbery, in consequence of the inter-
minable quarrels of their savage chiefs.   The royal autho-
rity, incapable of producing peace in these hereditary con-
flicts, was only, in the eyes of the ambitious vassals, an
instrument of dominion and of fortune.   We may add to
these, the reformed religion, which was now extending itself,
embraced by some from sincere conviction, but by a great
number from interest and fanaticism, while contrary senti-
ments and interests conspired with equal force to obstruct
its introduction.   At such a period, and in such a country,
it is not remarkable that, after two centuries, no trace of
the early history of a child should remain, notwithstanding
the high distinction which he attained in manhood.

Thus, notwithstanding the most active research, the
Scotch biographer has only been able to discover some vague
unimportant indications of the education of young Napier,
and, in order to fill up this void, he throws in a number of
tedious digressions, relating, for example, the more or less
doubtful biography of six or seven Napiers of Merchiston,
who preceded the author of the logarithms in direct line;
their fortunes, the alliances, political transactions, whether
commercial, military, or civil, in which they took part; and,
as is customary in Scotland, we find mention made of parents
of persons who then enjoyed great distinction; among others,
of the famous Bothwell, who married Mary Stuart by such
violent means; the author gives the history of Mary, Both-
well, and Darnley, while Louis XI., Charles le Temeraire,
figure in these digressions; and even certain persons are
more strangely associated with such materials as the page
Quentin Durward and the Abbe de la Deraison.   Then, as
young Napier appears to have passed some years at the
University of Saint Andrews, we have a history of the Uni-
versity, or rather, of the principal persons of the time who
were educated there.   From all this, we can only learn that
the inventor of the logarithms was descended from a rich,
old distinguished family, which had taken an inevitable but
reserved and prudent share in the political affairs of the
time.   Born at the castle of Merchiston in 1550, Napier
entered the University of Saint Andrews in 1563, which he
left some years afterwards for the purpose of travelling on

the Continent, probably to complete his education, as was customary among Scotch persons ofdistinguished rank. Having returned to Merchiston in 1571, he married in the following year; and, confining himself to his retreat, he divided his time between two chief occupations, the discharge of domestic affairs, with which his father had entrusted him, and mathematical and theological studies, for each of which he appears to have had an equal liking. But, in spite of his life of repose, he was too often forced to leave his asylum, either in order to escape the attacks of the military, or to take upon him the sole defence which his position and his religious opinions dictated, in the religious transactions of the time. There we can trace his actions, by the aid of numerous documents which his biographer has collected; and the study of the mental ideas which he carried upon the theatre of earthly affairs will not be superfluous in order to complete the philosophical view in which we ought to regard him.

It was at this time that the crisis of reform agitated Scotland most violently. James VI:, afterwards James I. of England, occupied the throne; a prince habitually weak, and sometimes possessing the power of exhibiting a certain firmness, not destitute of information, or rather, erudition, and rendering himself almost always ridiculous by his want of tact, in the selfish exhibition of his religious duties; tormented by the continual revolts of his stubborn vassals, by the daily increasing audacious exactions of the reformed party, whose puritanism distrusted him in consequence of his leaning to the Catholics; rendered uneasy, also, by the ambitious Elizabeth, who laid for him constantly a thousand snares, impatient as she was of beholding in him her direct and unavoidable successor, and one who was sprung from blood which her jealousy as a woman, and her politics as a queen, had shed.

In this state of peril and misery the poor King of Scotland remained for a long time, hoping always for some favourable change. Among these struggles of puritanism and royalty the Baron of Merchiston appeared upon the scene. He took part in those Presbyterian Synods which harrassed the King with indefatigable audacity with their fanatical exactions from the Catholics, whom, in their

opinion, they did not sufficiently persecute. Napier belonged
to the Synod of Fife, the most violent of the whole of them.
He formed one of the deputies whom the Synod, and then
the General Assembly at Edinburgh chose, to carry to the
King a solemn deliberation, by which it was declared that
" his faithful subjects irrevocably determined to risk even
their lives in order to be delivered from the idolatry and
the society of the Bloody Papists: that Lords Huntly, Angus,
&c., (here follows a list of the proscribed persons, among
whom is the father-in-law of Napier), by their idolatry,
heresy, blasphemy, apostasy, and enmity to Jesus Christ
and his church in this kingdom of Scotland, have cut them-
selves off from Christian Society, and thus, deserve to be
effectually excommunicated, separated from the Church of
Christ, and delivered into the hands of Satan, whose slaves
they are; until they learn, if it please God, not to blaspheme
Christ and his gospel," &c. Such were the holy pretensions
of the pious Presbyterians. It is to be observed that excom-
munication comprehended the confiscation of the property of
the wicked persons; property which devolved upon the
King to distribute among " God's Saints," as these worthy
people termed themselves. The poor King, in vain, made use
of the strongest and wisest efforts to prevent these shameful
proclamations from being carried into effect; he was obliged
to admit the deputies from the General Assembly into his
presence.   It is curious to observe, even in our day, the
traditional effect of the old puritanical spirit upon the mind
of the Scotch biographer.   He is delighted with the promi-
nent position given to Napier in these fanatical transactions:
" Our philosopher," says he, (p. 162), " must have been
particularly remarkable in this Assembly, (that of Edin-
burgh), which confirmed the excommunication of his father-
in-law."   (It was the father of his second wife, for he lost
his first in 1579).
    Then pursuing without hesitation the consequences of
this act, " If the family," adds he, " of Napier was present
at divine service, on the day when this was made public, his
own children must have tended to exclude their grandfather
from the benefits of the church, and of all the blessings
attached to human society."   Subsequently, he notices the
powerful effect which must have been produced on James,

by the appearance of the " majestic Napier, with his calm
aspect, his pensive eye, and his great beard, which the King
never before had an opportunity of seeing." Was not this
an essential merit to give to the inventor of logarithms,
and, especially, when connected with his discovery? But,
then, I may be asked, why do you cite these details, and
make these remarks? I make them, because in the obvious
intention of the biographer they have an object, and one,
which in my opinion, is contrary to the spirit of science
and of sound philosophy. This object is to exhibit the in-
ventor of the logarithms as a light of the Protestant Pres-
byterian Church, as the greatest theologian of his time, and
as principally a theologian, and this in order to support
religious belief by scientific discovery, to attempt under
this pretext to impose on us credulous exactions which
good sense repulses, and which, thank God, do not exist
in our time.

Undoubtedly, Napier was a theologian, a learned theo-
logian, and without doubt, his religious belief was com-
pletely sincere. This is due to his moral character. The
importance of the arithmetical invention, which we owe
to him is very great, as we have already had occasion to
remark. But, does it follow from this, that arithmetic
ought to make us receive his theology, and that it is neces-
sary with the Scotch biographer, to consider the commen-
tary upon the Apocalypse by Napier as admirable? for before
him, Newton had made a similar commentary, in which,
he undertook to prove that the Pope is antichrist, and
Christian Rome the prostitute of Babylon. Besides it was
not new at this period, since it was equally the favourite
theme of the fiery Presbyterian preacher, Knox, who called
the charming Mary Stuart a Jezebel; and King James VI.
himself had exercised his theological knowledge in proving
this point. It was at this time a current idea. But the
peculiar part which Napier took in this controversy, was
his having introduced a form of argument quite mathema-
tical; an order of discussion logically arranged; setting
down first a table of postulates, from which he proceeded
to interpret the divine figures; postulates which he took
care to establish, as well as possible, upon a number of
learned authorities. I shall not be so rash as to contest

such premises, nor even examine too punctiliously, if the
number, already very considerable, of the elements admitted
as bases, increased, in the course of discussion, by a sufficient
number of other hypothesis, has not very much weakened,
speaking in a worldly point of view, the mathematical pro-
bability of the final deductions. I admit, then, if necessary,
all this; confessing myself unable to dispute it; and I shall
thus be forcibly conducted to the necessary logical conclu-
sion that the Pope is certainly Antichrist; that he is also
Gog, as the emperor of the Turks is Magog, and his soldiers
the locusts of the Apocalypse. Besides that, there have
been twenty-two popes, horrible necromancers, who were
obliged to become perpetual slaves of the devil, in order to
become pòpes; as this is equally established in the book of
Napier, prop. xxv. The beast with two horns is antichrist
alone and his kingdom, p. xxvii. The pope alone is the
antichrist, particularly predicted by the prophets, p. xxxii.
Gog is the pope, and Magog, the Turks and Mahometans;
twenty-two popes, necromancers and slaves of the devil,
p. xxxvi. The locusts are the Turks.

But, among his conclusions, there is one which ought to
be equally indubitable, and which, by its logical connexion
with the others, evidently communicates to them its char-
acter of necessity. It is that, according to the 14th propo-
sition of Napier,. " *the Day of Judgment* ought to happen
between the years of our Lord Jesus Christ, 1688 & 1700; "
from which, according to the 10th proposition, " the end of
the world terminates in 1786, and rather before than after
it." This is a consequence of which I cannot, it is true,
contest the truth, as it follows logically from the premises,
but, I confess, that it appears to me difficult to receive it ;
and it is, perhaps, because it produces the same effect upon
other simple minds, that the Commentary of Napier upon
the Apocalypse is not read so frequently, at present, as
might be desired, as his biographer complains. Newton,
also, it is known, has commented on the Apocalypse, but
he has not undertaken so extensive a field as his Scotch
predecessor. " The folly of preceding interpreters," says
he, (folly is a strong word) " has been in wishing to predict
times and things by their prophecies, as if God had designed
to make them prophets." Thus, Newton confines himself

to explaining the past; and the greater number of persons who have read his work appear to have concluded that even this was not easy.

In giving an account of this Commentary of Newton, in the Universal Biography, I expressed some doubt upon the conclusion to which Newton has come, that " the 11th horn of Daniel refers to the Church of Rome." Dr. Brewster, in a work of the same kind, (I understand, it to be a work of the same kind as mine), published at London, in 1832, has reprimanded me for my facility of doubting, and has affirmed that this interpretation of the 11th horn, as well as others of the same kind, which Newton has given, may be developed, even to a full demonstration. I am, therefore, obliged to ask, humbly, of Dr. Brewster to be pleased to excuse, on this head, the impossibility which must exist in France, of receiving such Anti-Catholic conclusions. The Scotch biographer of Napier produces, with regret, the expression of repugnance which I have made, inasmuch as, according to him, the Commentary of Napier contains more than nine quarto pages of condensed proofs of this same proposition. Nevertheless, he wishes not to be offended at my blindness. " When M. Biot," says he, " states that he cannot believe the 11th horn of Daniel to be the Church of Rome, we are not surprized, in the present times; but it was otherwise in the time of Napier; and to this, we may add, that when Protestants such as Calvin and Scaliger, confess openly that they consider all the Revelation of Saint John as an inexplicable mystery, even the author being problematical, it is a great honour to Scotland, that, in the heart of a country so rude, such a commentary should be produced, worthy of the first erudition of the age, and capable, as we shall shew, of instructing even our more enlightened age." If we are allowed to appreciate this conclusion of the biographer, by human intelligence alone, I confess that I cannot see how it follows, from the authorities which he has cited, which rather appear to establish the contrary. But, perhaps, the character of inspiration in the text extends also to the panegyrist, in which case, I have no reply to make.

The Commentary on the Apocalypse was, on the part of Napier, an edifying work, and one produced after deep

study, which was undertaken for the purpose of converting
the Papists, as he states, himself, in the preface. But the
circumstances under which he chose to publish it, add to
his first project the character of a less charitable intention,
for that took place precisely two days after the demands
of the Presbyterians had forced from King James the definite
confirmation of the Act of Excommunication, in which the
father-in-law of Napier found himself included; and, in
the dedication of this commentary, to James, we can see
with what fanatical violence he talks of it : " Provided Sire,
that it may be the constant study of your Majesty, (as called
by God) to reform the whole misrule of his kingdom ; com-
mencing first (after the example of the royal prophet David)
by reforming his house and his heart, and purging them
from all communications with Papists, Atheists, and neutral
persons, of which this book of Revelations reveals that there
are a great number, and that they will multiply in these
last days.—Thus I supplicate your Majesty, that in weighing
and considering well the treasonable plots of this time,
attempted both against the truth of God, and against the
authority of your Majesty, and against the public welfare
of his kingdom," &c. " Commencing first with his own
person, and from that advancing to the reformation of his
family, and then to that of his heart," &c. Napier, in his
preface, explains himself the motives of this public oration.
" It was not my intention," says he, " to publish this work
suddenly, and still less to write it in our vulgar tongue;
until latterly, seeing the insolence of the Papists elevating
itself about the year 1588, and advancing and increasing in
this island, and moved with compassion and piety towards
those who gave more credit to the Jesuits and Priests of
the seminaries than to the Scriptures of God, and rather to
trust to the Pope and the King of Spain, (it was at the time
of the Armada), than to the King of Kings, to prevent the
evil which might follow, I gave up the Latin which I had
commenced, in order to produce, in common language, the
present book, scarcely yet complete, to instruct the simple
of this island, to overcome and frustrate the proud and
foolish attempts of the wicked : resolving, with the assistance
of God, to publish the Latin edition in a short time, for the
advantage of the whole church."

The Scotch writers who have brought these fine things *(belles choses)* to light at the present day, appear moved towards us with the same compassion which Napier bore to the Papists of his time. It is to be regretted that they have not at their disposal such advantageous temporary circumstances for favouring their doctrines. It was then the good times of sorcerers, sorceries, and burnings. Napier, according to the confession of his biographer, was supposed to hold conversations with *Old Nick;* and he even wished it to be thought that this opinion was not without foundation. But he was held in such high estimation that much disquietude was not felt in this respect. He appears to have been, in reality, occupied with mechanics and physics; for, when the English were afraid of a new attack from the Papists in 1596, Napier sent to the Scotch ambassador at London, a list of inventions, after the manner of Archimedes.*

These secrets are, burning mirrors, pieces of artillery on a new construction, and a new method of navigating under water; but all this is only announced, not described.

Unfortunately, he did not always make such a disinterested use of his science, as the following contract shews, which he formed with one of the most wicked men of the time, called Robert Logan, of Restalrig,—a contract written entirely with his own hand, and of which, the biographer has taken care to present a *fac simile.* This Logan, of Restalrig, had thrown himself, with ferocious audacity, into the desperate party of Francis Stuart, Count Bothwell, in 1594; and, under this title of open war, went about robbing and way-laying the roads in the neighbourhood of Edinburgh. The legality of these proceedings not being, unfortunately, recognised, he had been called to judgment and outlawed for his non-appearance. But this gave him little uneasiness; having, upon the wildest shores of the German ocean, an inaccessible retreat in Fast-Castle, since celebrated, under the name of Wolfscrag, by Sir Walter Scott, in the *Bride of Lammermoor*. There Restalrig, not knowing what to do, recalled to mind an old tradition respecting some treasures buried under his castle; and know-

* This list is contained in a letter to Anthony Bacon, entitled, "Secret inventions, profitable and necessary in these days for the defence of this island, and notwithstanding Strangers, enemies to God's truth and religion," Dated June 2nd, 1596.—EDIT.

ing Napier to be a learned man, and something of a necro-
mancer, he proposed to him to engage in the discovery,
which the latter undertook, as may be seen, in good faith
and honour, under the titles and clauses in the following
contract, which we translate from the Scotch text:

" At Edinburgh, the — day of July, the year of our
Lord, 1594, it is appointed and agreed between the under-
signed persons, that is to say, Robert Logan, of Restalrig,
on the one side, and John Napier, holding the manor of
Merchiston, on the other side, in the form, manner, and
deeds, as follow, viz. Forasmuch as there exist divers
ancient traditions, reasons, and appearances, that there
is in the dwelling of the said Robert, at the place called
Fast-Castle, a sum of silver money and treasure, secretly
deposited and concealed, the whole of which has not been
discovered by any one : the undernamed John will make
all possible and exact diligence to seek for it and find it
out, and endeavour to extract the sum in question : and,
by *the Grace of God,* either he shall find the said sum, or
he shall assure himself that the like deposit has not been con-
cealed there : the whole, as far as his labour, diligence, and
his science, can assist him : For which the said Robert shall
give, and, according to the tenor of the present writing,
gives and grants to the said John, the exact third of what-
ever silver, or treasure, the said John shall find, or which
shall be found by his means and industry, in the said place
of Fast-Castle, or its neighbourhood : And this to be
divided, by just weight and balance, between them, without
any fraud, opposition, and contention whatever, in such a
manner that the said Robert shall have fully two parts,
and the said John justly the third part of the whole, upon
their faith, word, and conscience : And for the sure return
and safety of the said John, from the above mentioned
place of Fast-Castle to Edinburgh, without being robbed
of his third part, as well as without receiving any damage
in his person, or in the effects which belong to him, the
said Robert shall convey in safety the said John, and
accompany him whole and safe, in the manner above men-
tioned, to Edinburgh : To which place, if the said John
shall return without difficulty, it will behove him, in
presence of the said Robert, to efface and destroy the pre-
sent contract, as a full discharge of the two parties having

honestly accomplished and fulfilled their engagement the
one to the other : and it is decided that any other discharge
than the destruction of the present contract shall not be of
any value, force or effect : And, in case the said John shall
not find any treasure, after all his efforts and diligence, he
shall rely for the disbursement of his work and difficulties,
upon the the discretion of the said Robert. In witness of
the present, and in testimony of all honesty, faith and fide-
lity, to be observed in all its conditions, relative to each of
the two parties, they have both subscribed the present with
their own hands, at Edinburgh, day and year as aforesaid.

<div align="center">

(Signed)   " ROBERT LOGAN, of Restalrig.

" JOHN NAPIER, of Merchiston."

</div>

How could the great theologian of Scotland, the *Wonder-
ful Napier*, as his biographer terms him, conscientiously
contract such an engagement, and an engagement almost of
necromancy, with a declared robber and assassin ; he who
evidenced so much horror, and such a scrupulous indigna-
tion against the temporal excesses of Papists, and against
those twenty-eight Popes who were decided necromancers ?
The biographer does not dissemble the difficulty of this
question, and attributes the act to the barbarous rudeness
of the times, and *to the simplicity of mind of our philo-
sopher*. In our opinion, a more true and more grave ex-
planation might be found in the doctrine admitted then in
Scotland, among the Casuists of the Puritan Confederacy,
and renewed at the present day by another sect, which ap-
pears to be making great progress in England ; it is, that
all means are good for Saints ; in other words, that Saints
do not sin. The Scotch biographer passes, in detail, over
the moral consequences of the act, and only takes occasion
to admire the " unconquerable courage of the man who
feared not to engage alone with a robber in his cave."
After which, he adds, " To pronounce this transaction as
mercenary, would be to apply a false appreciation of modern
notions to manners obscurely appreciable by antiquity."
Papists, then, are not the only persons who have accomo-
dating opinions.

Here terminates what we have to say of Napier, as a
politician, moralist, and theologian. We have explained
above, the considerations which have induced us to study
his character under this point of view, according to the

numerous data furnished by the author of the new biography. It remains now for us to consider him as a mathematician, and thank God our task will be henceforth much more easy; for, the method of re-calling him to memory in this point of view, the only one which, in our opinion, merits the attention of posterity, will be to make extracts from his own works, completed by several new and curious documents which his new biographer has added. In this respect we may say, with justice, that this biography has been highly useful.

(To be continued.)

ARTICLE II.

On the Number and Character of the Colours that enter into the Composition of White Light. By PAUL COOPER, Esq.
(Concluded from p. 64.)

IT is a well known fact that, with common light, refraction is always attended by partial reflexion ; and it is equally well known that the internal reflexion from glass, is stronger than the external; even under ordinary circumstances this reflexion is considerable, and with such intense light as the direct light of the sun, at a large angle of incidence, such as it is in the present experiment, it must be sufficient to produce very decided effects. The question, then, is, Do these reflexions, traced upon optical principles, correspond with the intermediate images discovered in the spectrum ?

This is the question which I shall now endeavour to answer :   Let A B C (*Fig.* 3) * represent the section of an equilateral prism, perpendicular to the axis, and E F G H a ray of homogeneous light, incident upon the centre of the surface A C, at such an angle that F G may be parallel to A B, and, consequently, B G H equal to A F E ; in this case it is evident that the first internal reflexion will be from G to D, the second from D to F, and the third from F to G, where it will fall at the same angle of incidence as the direct ray F G, and, of course, be equally refracted upon emergence in the direction G H.   The fourth reflexion will take the same direction as the first ; and, proceeding in the same order, the sixth will coincide with the third, forming upon emergence at G, with the direct ray, and rays from

* See page 59.

any subsequent reflexions, multiples of three, a single image, by superposition, at all distances from the prism.

But, if we suffer a ray of white light, E F (*Fig.* 4) to fall in the same direction upon the point F, only one of the three colours of which it is composed can pass through the prism parallel to A B; the others will be dispersed, supposing *g* to be the green ray, the violet to 1 *r*, and the red to *r*.

The angle of dispersion at the internal surface B C is only half what it would be upon emergence, after a second refraction, and too small to be correctly represented; we may, however, trace the different reflexions upon an assumed scale, as in the figure, so as to give an idea of their relative positions, and we shall thus find that the violet ray will emerge, after three reflexions, below the green ray, at an angle with a line parallel to it, equal to the angle of dispersion, but diverging from it in an opposite direction; and that, after six reflexions, it will emerge parallel to the direct violet ray, but above it.

Now, as there must be some breadth of light to form images of the different colours, and, as the space between 1 *v* and 4 *v*, though differently represented in the figure, is, in fact, very small; if the light emerged at the same angle it would form only one elongated image; but, by its emission at different angles, the divergence of the direct and the reflected images is continued after their emergence, and at a sufficient distance, they must be completely separated.

The chief, if not the only difficulty that presents itself in this arrangement, is, that the reflected violet ray, 4 *v*, being nearly parallel with the red ray, *r*, ought, it might be supposed, to blend with it in preference to the green ray, *g*, but this is a difficulty in appearance only; for, supposing the rays 4 *v* and *r* to be parallel, and, consequently, to have the same angle of incidence upon the surface B C, the difference of refrangibility in violet and red light, would cause a rapid divergence of these rays after a second refraction; and they must, therefore, form distinct images, at a short distance, after their emergence: this being the case, and it being equally impossible that the ray 4 *v* can blend with the ray 1 *v*, it must either form a junction with the intermediate green ray, *g*, or emerge separated from both; the latter

would no doubt be the case with a single ray of each, but with a breadth of light, such as would be required to make the experiment, the divergence would, in all probability, be too small to effect the complete separation, except at a considerable distance, the greater refrangibility of the violet light being compensated by the greater refracting angle of the green.   Hence, the blue formed by the junction of the two rays, cannot be separated by methods which succeed when the rays intersect each other at a greater angle.

If we now refer to the violet ray after six reflexions, *v*, we may observe that it is parallel to the ray 1 *v ;* and, therefore, as the space between *v* and 1 *v*, if the figure were correctly drawn, would be less than the breadth of light necessary to make the experiment, the two images must be partly superposed, and, being formed of parallel rays, must continue so at all distances : the same will hold good of images formed after twelve reflexions, or any other multiple of six ; and the whole will emerge at the same angle, one above the other, and form one lengthened image only.

Hence the extension of the spectrum in strong light, a circumstance not accounted for in the Newtonian theory; and hence, probably, the chemical rays, when the reflexions are too weak to be visible.

By tracing the reflexions of the red ray we shall find every circumstance attending it giving support to the same views.

The white light E F, (*Fig.* 5) is dispersed by the refraction of the surface A C, into violet *v*, green *g*, and red 1 *r ;* the green ray being, as in the former figure, parallel to the base of the prism A B : the red ray, after three reflexions, emerges at 4 *r*, at a greater angle of refraction than the direct ray 1 *r*, and, supposing the angle of dispersion formed by the violet and the red rays with the green ray equal, parallel to the violet ray; consequently, and for the reasons assigned with regard to violet light, the ray, after three reflexions, will form an image in the spectrum above the red and below the violet images; it must, therefore, either be a distinct image, or be blended with the green ; and, if with the green, with the lower part of it; for, although, to render the subject more simple, we have assumed the dispersion of the violet and red rays, from the mean ray, equal,

it is well known that the dispersion of the red light in glass is less than the violet, and, therefore, the red ray after three reflexions will be less elevated than is here represented.

The red ray after six reflexions, will emerge below the direct ray, and parallel to it; so as to form with it, and any other reflexions, multiples of six, an elongated image in the spectrum; but as the angle of dispersion in red is less than in violet light, the reflected rays approach nearer to the direct ray, and the elongation of the red light is less considerable; the images being in a greater degree superposed.

We thus account for the formation of blue and yellow light, by the intersection of rays nearly parallel, though of different refrangibility; we give a probable reason for the presence of chemical rays at one end of the spectrum, and heating rays at the other; both being produced by reflected images, invisible from their low intensity, when, if our preliminary observations be correct, the light is more readily absorbed; we also account for the increased elongation of the spectrum in strong light; and for various appearances in it, when subjected to the action of absorbing media.

Such a concurrence of circumstances can scarcely be attributed to mere chance; and, connected with the considerations previously advanced, although it may not be sufficient to prove the truth of our hypothesis, at least gives it a claim to farther inquiry. It must be observed, however, that although, upon further investigation, this hypothesis may fail to account for those appearances in the spectrum, which may be supposed to support the idea of innumerable degrees of refrangibility, it will not by any means invalidate the proofs that are advanced in this paper of the erroneous character of this doctrine, which are perfectly independent of it; the only difference this failure would make, would be, that we should have to direct our inquires to other probable causes; and several readily offer themselves to our consideration.

When the spectrum is formed by suffering the unobstructed light of the sun to fall on a prism, and its coloured images fall perpendicularly on a screen, there will be observed a disposition in the red part of the spectrum, particularly in the lower part of it, to assume a circular form; and upon looking at the spectrum through coloured glasses,

not only the red, but, also, the green and the violet images, may be seen of this form, quite separated from each other, and without the appearance of any intermediate light. I have not yet seen the three images together by this method, but the green and red, and the violet and red, may be made to appear at the same time, without difficulty.

In making these experiments, it will be observed that the red image assumes its circular form in consequence of the absorbtion of another red image immediately above it; this red image, which is removed with great facility by means of blue glass, is, probably, formed by the red light after three reflexions.

Now, all these circular images of the sun, in both methods of making the experiments, must be formed of homogeneous light. What then becomes of the innumerable other circles formed by the intermediate rays? Surely it will not be said that these are absorbed, leaving only one of each colour. I will not attempt to answer such an objection, because I do not think it will be made; and, as I see no other that can be made, the conclusion, that there are only three colours, and three degrees of refrangibility, appears to me to be obvious.

Upon the same principle, and by the same means, that circular bodies, when seen through a prism, produce as many round images as there are homogeneous rays, correct images of the different forms of other bodies may be produced; and the number of these images depends, in like manner, on the number of homogeneous rays which they emit, or by which they are illuminated.

If we look through a prism, held vertically, at the flame of a candle, it produces a spectrum, which increases in length by increasing the distance of the prism; and if, when at a sufficient distance, we interpose coloured glasses, we may obtain images of the flame of the candle, which in every respect, except in colour, correspond with the original, and with each other, in red, green, and violet light. I do not recollect producing the three images together, but they may be produced either separately, or in pairs, with the greatest facility, and with perfect correctness; the wick of the candle, and every circumstance attending it, being as distinct as in white light, when viewed by the eye without the prism.

There is an argument in favour of a limited number of degrees of refrangibility, if arguments were necessary where facts are so abundant, which deserves consideration. The rays of light of different colours, I have already observed, are independent of each other ; and this independence is so complete, that in every part of the retina there is a distinct channel of communication for each colour, which, when the ray it is designed for is absent, remains unemployed.*

* This view of the subject will satisfactorily account for the production of a black accidental image, by the alternate action of complementary colours, viewed in the manner proposed by M. Plateau, [6.*] whose method of producing this effect I was unacquainted with at the time it was written; if we view crimson and green, or any other complementary colours alternately, the effect, to be consistent with these observations, ought to be the same as when we look upon both colours together, as they are reflected from a white object.

I cannot see, in this experiment, any thing opposed to the theory of accidental colours most generally admitted, as it is, I think correctly, stated by M. Plateau at the commencement of the paper [1.]

I admit that if what follows [2.] be really a fact, " that accidental colours may be seen in *perfect darkness,*" this theory is unsatisfactory ; but I am not acquainted with any clearly established case of the kind. I confess, however, that from the defective state of my eyes, I have been unable to make some experiments which would enable me to speak more decidedly on this point.

White light, so weak as to be imperceptible in this state, is frequently rendered visible, and, in some cases even brilliant, by withdrawing either one, or two, of its constituent colours. This may be proved by several experiments that are familiar to us.

The production of colours, on the pressure of the eye-ball, and in several cases of the disease of the organ, as well as those cases where its appearance is altogether the work of the imagination, ought, in my opinion, to be excluded from this class of phenomena ; we otherwise confound appearances which have a totally different origin. These colours are generally uncertain ; whereas, those which are, I think improperly, termed accidental colours, are always complementary to the primary colours, *with reference to the light with which they are seen*; and, of course, when this light is white, the accidental are truly complementary colours ; generally, however, of less intensity than the primary colours ; because the sensibility of the eye to the latter, though lessened by its previous action, is seldom, in these experiments, entirely removed, and the accidental colour is, consequently, diluted with white light.

In cases where the accidental colour is seen with light of a much lower intensity, as, for instance, when the eyes are closed after being impressed with the primary colour, the eye appears to be totally insensible to the latter, in its reduced state, and the accidental colour is thus rendered very brilliant.

Black accidental images may be accounted for on the same principle : [4] the eye is rendered less sensible to all the colours by the contemplation of white light, and insensible to light of a much lower intensity ; when any part of it, therefore, is impressed with strong white light, and the eye is then exposed to weaker light, that part of it, thus impressed, is insensible to it, and forms a corresponding black

* Records, vol. ii. p. 281.

A little consideration will convince us, of the necessity of such an arrangement; for, if the atoms of light of different colours mixed indiscriminately with each other, which would be a necessary consequence of their being transmitted by a path common to the whole, it would be impossible, after the numerous intersections which perpetually occur, that light should re-appear, when proper means are taken for its separation, precisely in the state it was at first produced. Hence it is, that when the sensibility of the eye has been impaired by the action of light of any particular colour, its complementary colour, when presented to it, produces a vivid sensation; and when both colours are presented together, in the form of white light, the effect of the latter, communicated through channels hitherto unemployed, gives such a decided preponderance to the sensation

image; if the light to which the eye is exposed be of equal intensity, the image is gray, or black diluted with white.

The lessened sensibility of the eye, when impressed to light of equal intensity, and its total insensibility to light of a lower intensity, is an important distinction, in strict analogy with our other senses, and verified in a great number of instances; it is upon the latter principle, that weak light, such as shadows, the reflexion from the first surface of transparent media in Mr. Tomlinson's experiments, &c., so readily assumes the complementary tint of the colour to which the eye has been previously exposed.

Accidental colours of impressions destroy direct corresponding impressions, [5] by converting the white light, which generally accompanies the distinguishing colour, into the complementary colour of the object; by which means, the whole is reduced to *weak* white light, or assumes an appearance between black and white. In some cases, an instance of which, is mentioned in page 178, the accidental colour, thus produced, is more than sufficient to neutralise the direct light, and the object then assumes the complementary tint. (See introductory remarks on the Effect of Contrast, p. 177. See, also, the conclusion of the inferences drawn from the subject to which this note has reference, which presents the same principle in a different form.)

The same principle, I apprehend, applies to the explanations of Sir David Brewster's experiment, with the red wax, described in page 183. The lessened sensibility of the eye to the colour of the wax, by its previous action, is rendered total by the stronger light of the candle; and the rays of the other colours, which, in this case, as well as almost every other, accompany it in the state of white light, are thus rendered visible under the appearance of a weak phosphorescent colour.

The simple theory which I have advocated, accounts so readily for a large class of phenomena, extended by including the effects of contrast; and it bears such a striking analogy in its application, to the known operations of our other senses, that I cannot surrender it without much stronger proofs of its inadequacy, than any which I am yet acquainted with.

I am glad the subject is about to undergo a thorough investigation, by a philosopher, whose recent discoveries connected with it, lead us to anticipate much valuable information.

produced by its own colour, that the white light appears
wholly converted to it : it is, in fact, no longer white light,
but the complementary colour, diluted with white light in
proportion to the state of sensibility in which the previously
employed parts of the eye meet the primary colour;
white light in this, and in every other instance, is formed
not in proportion to the quantity of light of the different
colours present, but in proportion to the sensations pro-
duced by them.

Now, if a distinct means of communication be necessary
for each colour, in every point of the retina upon which a
ray of white light falls, which, from various considerations,
appears to me to be very evident, how is it possible such an
arrangement could be made if the colours, instead of being
limited to three, were innumerable ?

But, what renders the doctrine still more suspicious, is,
that it is unnecessary; for, according to this hypothesis,
all these simple colours may be imitated by a composition
of neighbouring colours; so that, it would appear, nature
has provided two causes, or distinct methods, to produce
one and the same effect; a prodigality of which, I believe,
we have no other instance.

In conclusion, I beg, Sir, to apologize for having occupied
so many of your valuable pages; the title I have chosen for
my paper has given me such an ample scope, that, though
I fear I have already been tiresome, I might have greatly
extended the subject without passing its boundaries.   The
character of light, in its most extended sense, would include
almost every branch of Science; great and admirable as are
the properties by which it is made the medium of our com-
munication with distant objects, I have no hesitation in say-
ing, that its application to other purposes, is of even higher
importance; the whole system of nature seems to be depen-
dant upon it; and its material qualities, which modern
philosophers, from having confined their attention to a very
limited part of the subject, have dismissed as useless, will
be found essential in every part of her economy.

The theory upon which these views are founded, claims
my confidence, by connecting, upon the most simple princi-
ples, the whole phenomena of nature ; whether displayed
by her ordinary means, or elicited by the aid of experiment.

It is not, therefore, upon light grounds, that I have ven-

tured to give an opinion with regard to the material cha‑
racter of light, so decidedly opposed to the present prevailing
theory.   I am too well aware of the ease with which a
plausible hypothesis may be formed to meet a limited class
of facts, to place any reliance upon those numerous specula‑
tions which obtrude themselves upon the theorist, in the
course of his investigations, and which, from their equal
pretensions, frequently form one of his greatest difficulties;
but, when a theory presents itself which enables me to trace
the different operations of nature with the same ease that I
follow the train of a well connected piece of machinery;
when, assuming the principles of this theory, I am led to
discoveries, which so correctly correspond with the results
of these operations, as to convince me they are its necessary
consequences; when, again, after a lapse of many years,
during which numerous discoveries have been published, I
find the whole of these discoveries either previously attached
to it, or readily included in it without the slightest altera‑
tion, I can no longer doubt its claim to my confidence, or
hesitate in giving an opinion under its sanction.

The chief support of the undulatory theory, arises from
an erroneous conclusion, that the interference of light must
necessarily be a destructive interference.   I freely admit,
that if the annihilation of light were satisfactorily established,
the material theory must instantly fall to the ground; but
something more than the gratuitous assumption of the fact
is required to prove it.   I am prepared to show, that in
cases of interference, the light never arrives at the points
where it is supposed to be destroyed; and that the cause of
this is connected with a property of light of the most exten‑
sive utility.

I must, observe, however, that a great part of those cases
of the production of colours, which have been attributed to
interference, may be accounted for upon the principle of
refraction; and, with your permission, I propose giving
some instances of this, connected with other subjects, in a
future communication.

In the mean time, I hope the subject of these papers will
not be considered unworthy the attention of some of your
correspondents :   There is scarcely a modern treatise on
optics that does not give Newton's Scale of Colours as the
foundation of the science; and, perhaps, in some other part

of the work, states that other philosophers, among which, the author is in some instances included, have adopted opinions opposed to it; this contradiction, which must necessarily extend to the branches connected with this important part of the subject, not only indicates a state of uncertainty in science, which it is desirable to remove; but it also presents to the inexperienced reader a source of difficulty, which must considerably impede his progress; on both these accounts, and for many other reasons which have been stated in the progress of this inquiry, the question claims our consideration.

PAUL COOPER.

Bawlish, 25th November, 1835.
To the Editor of the Records of General Science.

ARTICLE III.

*Water of the Elton, Dead, and Caspian Seas.**

THE Elton Sea lies to the east of the Volga, 274 versts (181½ miles), south from Saratov. Its greatest diameter, from east to west, is 17 (11¼ miles), and its smallest diameter 13 versts, (8½ miles).

The specific gravity of the water, at 53½°, is 1·27288, according to Rose.

Its contents are according to Rose and Erdmann:—

	ROSE.	ERDMANN.
Chloride of sodium, . . .	38·3 . . .	71·35
Chloride of potassium, . .	2·3 . . .	
Chloride of magnesium, . .	197·5 . . .	165·39
Sulphate of magnesia, . .	53·2 . . .	18·58
Sulphate of lime, ...	. . .	·36
Sulphate of soda, ...	. . .	3·84
Carbonate of magnesia, . .	. . .	·38
Water and organic matter, .	708·7 . . .	740·10
	1000·0 . . .	1000·00

When the temperature of the sea falls, Epsom salt precipitates. Here it is evident that the specific gravity and composition must change with the temperature. The shore of the Elton sea exhibits, in summer, crystals of gypsum and common salt; and, in winter, besides these, Epsom salt, which, in summer, is again dissolved, so that pure

* Poggendorff's Annalen, xxxv. 169.

common salt may be obtained here. In the cool sum-
mer nights, according to Pallas, Epsom salt is deposited,
and is again dissolved during the day. The greater the
quantity of chloride of magnesium and Epsom salt, so
much the less is there of common salt; which, from the
elevation of the temperature, dissolves in no greater quan-
tity in the same. Hence, the reason for the small quantity
of common salt which Rose obtained. When an analysis
of such a saturated water is given, it is absolutely necessary
to give the specific gravity and the temperature. The
reasons given are sufficient to account for the difference in
the two analyses.

Erdmann found the constituents of the Bogden sea,

Sulphate of lime, ...	·74
Sulphate of magnesia, ...	10·30
Sulphate of soda, ...	215·76
Muriate of lime, ...	8·85
Muriate of magnesia, ...	48·63
Water, ...	715·72
	———
	1000·00

The water of the Elton sea resembles that of the Dead
sea, but the latter has a less specific gravity, and a smaller
quantity of solid constituents. The quantity of salt dimi-
nishes when the Jordan is overflowed. Gay Lussac allowed the
water to cool to 19°·4 F. without separating any salt. While
Klaproth states, that at the bottom of the flask which con-
tained the specimen which he examined, crystals of com-
mon salt were deposited which soon disappeared. The
specific gravity of the Dead Sea varies, and the reason is
obvious. Macquer, Lavoisier, and Sage found it 1·240;
Marcet and Tennant 1·211; Klaproth 1·245; Gay Lussac
at 62°·6, 1·2283; Hermbstädt at 60°, 1·240. The propor-
tion of ingredients also varies. Gay Lussac found them
26·24 per cent.—consisting of chlorides of sodium, cal-
cium, magnesium, and potassium, and traces of gypsum,
differing from that of the Elton Sea by the absence of
Epsom salt, and the presence of chloride of calcium.

According to Marcet, the specific gravity of the water of
the Sea of Urmia is 1·16507, and its constituents 22·3 per
cent, consisting of common salt, Epsom salt, and sulphate
of soda. The saline contents of Urmia and the Dead Sea

are, therefore, inferior to those of the Elton Sea. Rose has appropriated all the sulphuric acid to the magnesia, because he has found that when common salt and Epsom salt are dissolved in a sufficient quantity of water and evaporated in a summer heat, the two salts separate; and when much common salt is dissolved along with a small quantity of Epsom salt, a part of the common salt separates first, and then the Epsom salt, while common salt remains in solution; as by the heat of summer, Epsom salt is less soluble than common salt. When the temperature is raised above 122° F., or sunk to zero, in both cases, glauber salt and chloride of magnesium are formed.

Rose found the specific gravity of water brought from the Caspian sea 75 versts from the islands formed by the Volga, at 54½°, 1·0013; and its contents,

Chloride of sodium, ...	·754
Sulphate of soda, ...	·036
Sulphate of lime, ...	·406
Bicarbonate of lime, ...	·018
Bicarbonate of magnesia, ...	·440
Water with a small quantity of organic matter,	998·346
	1000·000

ARTICLE IV.

The Action of Isinglass in clearing Malt Liquor explained.
By MR. SAMUEL ROBERTS.

IN explaining the action of Isinglass in clearing malt liquor, two subjects present themselves for particular consideration.

The first is the nature and properties of Isinglass. The second is the change which takes place in malt liquor during the process of fermentation.

The best Isinglass is obtained from the sounds of the fish of the genus Accipenser, especially from the Sturgeon, found in the Danube, and the rivers of Muscovy. It is also obtained from the sounds of the Beluga, and Huso Germanorum.*

Isinglass is almost entirely gelatine, 98 parts in every 100 of good isinglass being soluble in boiling water.

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* Very pure Isinglass is also procured from the American fish, *Gadus merluccius.* The long stripes of Isinglass met with in commerce, are from the *Gadus morrhua.* See *Records*, vol. i. 239.—EDIT.

The properties of gelatine must therefore be considered, it being analogous to pure Isinglass.

Gelatine is distinguished from all animal principles, by its ready solubility in boiling water, and also in most of the diluted acids, which form excellent solvents for it.

Gelatine is perfectly insoluble in alcohol, and almost equally so in cold water. It is precipitated from its solutions by infusions of tannin.

An infusion or tincture of galls, will precipitate it from its solution in 5000 times its weight of water.

These are briefly the properties of gelatine, or pure Isinglass, which should be borne in mind in operating on it.

A great variety of Isinglass is offered for sale, at a range of prices from three, to sixteen shillings per pound; and the relative value of each kind may be known by the following tests:

In the first place, Isinglass should remain unchanged by being steeped in spirit of wine or alcohol, from 50° to 60° over proof, in which gelatine (the chemical principle of Isinglass) is insoluble. The alcohol, or spirit of wine, in which the isinglass has been steeped, should then be tried with a few drops of tincture of galls; if the liquor remain clear and unchanged, it is much in favour of the character of the Isinglass. If, on the other hand, the tincture of galls causes a precipitate from the alcoholic liquor, the Isinglass is not pure, as it contains something more than pure gelatine.

Different samples of Isinglass which have remained unchanged in alcohol or spirit of wine, should also be tried by the two following methods, before an opinion can be given as to their relative value. Try given weights of each sample (one-eighth of an ounce for instance) in three ounces of water by measure) in separate vessels; bring them gradually to a boil, occasionally stirring each sample. While hot, strain the different solutions through muslin, into separate vessels. In proportion to the quantity of undissolved matter left upon each strainer, may the solubility of the different samples be ascertained; that which leaves the least residuum will form, when cold, the strongest jelly, upon which the clearing property of Isinglass depends. The remaining trial to which the different samples are to be submitted, is the last and most decisive one. Equal weights of each sample

(the one-fourth of an ounce, for instance), are to be cut into very small pieces, and each one-fourth of an ounce put into half-a-pint, (imperial measure) of hard or sour beer, and the several vessels containing the different samples put into an apartment, at from 65° to 75° F., and allowed to remain there for three days, stirring each sample very well, once or twice a-day.

At the expiration of that time, there will be an evident difference in the strength of each jelly, provided different qualities of Isinglass had been submitted to the experiment, and when the thickest jelly has a small quantity of the tincture of galls applied to it, and stirred through it, it will separate the gelatine from the sample of Isinglass in the form of a thick jelly. The other samples which afforded a less solid jelly, will give, with tincture of galls when stirred through it, a smaller quantity of gelatine in the form of thick jelly.

From the strength of the jelly given, by any sample of Isinglass steeped in the above proportion of sour beer, (such as brewers use in making clearings), and submitted to a temperature *not exceeding* 75° F., may be ascertained the relative value of that sample, as upon the strength of the jelly, and, consequently, the quantity of gelatine contained in any Isinglass, depends its value in clearing malt liquor. The best short-staple Isinglass is always soluble in boiling water to about $\frac{1}{30}$ residue.

In the preparation of brewers' clearings, Isinglass, of a good quality, is steeped in acid beer, in an apartment of about 50° F. temperature. After some time, the Isinglass is converted into a jelly by the acidity of the hard beer, it being one of the qualities of gelatine to be soluble in dilute acids.

An advantage arises to the brewer, in always making his clearings from sour beer of an uniform strength of acidity, by which means he is protected from any disappointment resulting from the strength of the clearings he uses, provided he was previously aware of the good quality of the Isinglass, he submitted to the action of the sour beer.

A simple method may be taken to try the acidity of malt liquor, of which a brewer intends to make clearings. Make a standard liquor of one part (by weight) of the bi-carbonate of potash, dissolved in sixteen parts (by measure) of water.

Suppose, for example, that sixteen fluid ounces of the acid porter to be tried, is put into a vessel that will contain about double that quantity.

Carefully measure a given portion of the standard liquor, say four fluid ounces. Add small quantities of this standard liquor to the acid porter (stirring the mixture upon every addition), until the effervescence ceases, or until the mixture is so neutralized by the standard liquor, as not to change the colour of litmus paper when it is dipped in. The mixture may further be tried by turmeric paper, which should be changed to a higher colour by the mixture, thereby showing that the alkali of the standard liquor is slightly in excess.—By the quantity of standard liquor required to produce this result, may be known the greater or less degree of acidity of the porter to be used for making clearings.

Sixteen fluid ounces of acid porter, such as is used by one of the largest breweries in Ireland for making clearings, standing 1° Twaddle's hydrometer, 61° F. temperature, required four fluid ounces of standard liquor to neutralize it, which is equal to 1 part bi-carbonate of potash to 64 parts of acid porter.

This appears a good average strength of acidity for porter, when required for making clearings. If acid porter required less standard liquor to neutralize it than the above quantity, it would indicate a weakness of acidity, which would render such porter an imperfect solvent for Isinglass.

Acid porter, such as the above trial was made with, acts upon Isinglass, at 61° F., but its action is much facilitated by an increase of temperature of 80° or 90° F.

During the fermentation of malt liquor, the saccharine matter of the malt is gradually converted into alcohol, by the agency of yeast and atmospheric air; ultimately the liquor passes from the vinous into the acetous fermentation.

This latter state is prevented by the exclusion of atmospheric air; hence, the necessity of bunging securely malt liquor, when the vinous fermentation is complete, otherwise the liquor will become sour.

It is when unfined porter is put into casks, and the vinous fermentation has, either in part or entirely, ceased, that brewers apply the clearings to the best advantage. The manner in which Isinglass acts upon unfined liquor in clear-

ing or fining it, is by two properties of gelatine (the chemical principle of Isinglass). First, its solubility in weak or dilute acids; and, secondly, by being perfectly insoluble in alcohol, and sparingly soluble in cold water.

When clearings, or isinglass, in combination with sour or acid beer, is applied to malt liquor in a state of vinous fermentation, the alcohol of the liquor disengages the gelatine of the Isinglass from its solution in the acid porter, and, being thus liberated, it carries with it the impurities of the liquor which were suspended in it.

The following experiments will better illustrate the theory:

Mix a small quantity of brewers' clearings with cold water. In a short time the greatest part of the Isinglass will be separated.

Filter the mixture through paper, and if a few drops of tincture of galls be added to the filtered liquor, a small portion of gelatine will be precipitated.

This shows that the acid of the clearings held a small quantity of the gelatine in solution. This is further proved by adding to the filtered solution as above, a few drops of liquor of ammonia, instead of tincture of galls. The liquor of ammonia should be cautiously added in sufficient quantity, to neutralize the acid contained in the clearings, when the gelatine, which was previously held in solution by the acid, is liberated.

By this experiment is shown, the solubility of Isinglass in acid porter, and nearly its total insolubility in cold water.

Again, dilute a small quantity of clearings with cold water, until its acidity is so far overcome, as not to redden litmus paper, when applied to it. The Isinglass of the clearings will be completely separated from the mixture; for, if a few drops of the tincture of galls be added to the filtered solution, it will remain unchanged.

Add warm water, at about 180° F., to another small portion of clearings, until the liquor is so much diluted as not to affect litmus paper when dipped in it, as in the previous experiment. Filter the liquor through paper, when cold. Gelatine will be detected in this solution by adding a few drops of tincture of galls. This shows the solubility of Isinglass in hot water.

The next experiments will more clearly show the influence of alcohol in separating gelatine from its solution in weak acid, by which its action in clearing porter in a state of vinous fermentation, is explained.

To sixty parts of cold water, add one part of alcohol, and into this mixture put a small quantity of clearings.

In a short time the Isinglass will be separated from the mixture, and the liquor will be clear and bright.

If, however, the liquor be filtered, and a few drops of tincture of galls be added to the filtered solution, a small portion of gelatine will be disengaged.

This is caused by the free acid of the sour porter, of which the clearings are made, holding this small portion of gelatine in solution, as it may likewise be separated by adding a few drops of liquor of ammonia, instead of tincture of galls.

Reverse the experiment by using hot water at 180° F., instead of cold, adding the same quantity of alcohol and clearings, as in the former experiment.

After the mixture has been allowed to settle for a few hours, it will be found that there is but a partial separation of the Isinglass, and the liquor will not be so clear as in the former trial, the reason of which is, that the hot water dissolves, and retains in solution a portion of the Isinglass of the clearings, the remaining part being separated by the alcohol, in which it is insoluble. The effect would be exactly similar upon malt liquor, if clearings were applied to it which had been made with an *excess* of *acid* beer. The alcohol of the unfined porter would disengage but a small portion of Isinglass from the clearings, the remainder being held in solution by the excess of acid in the porter, the disengaged gelatine carrying with it particles of vegetable matter, which, when suspended in the liquor, rendered it but semi-transparent.   S. R.

ARTICLE V.

Notice of some Recent Improvements in Science.

I. ACOUSTICS.

Nodes of vibration.—By comparing the theory of musical strings and rods with that of musical pipes, Weber has

endeavoured to bring the latter to the same state of advancement as the first. In regard to the theory of strings and rods, he has brought forward several new facts. In musical rods, hitherto, the situation of the nodes of vibration, was only known by means of empirical rules laid down by Chladni. These gave a very uncertain result, so that Chladni recommended in acoustic experiments, the tight application of a soft roller of cork or caoutchouc to each rod, in order to produce a pure note. Weber shows,* that Euler has given an equation which exhibits in all cases the situation of the node of vibration, and which, is to be employed for fastening the rods, so that they give pure and powerful notes. By this equation, the situation of the node of vibration is shewn to be for

The fundamental tone (*grundton*) 0·2240 from each end.

The first falsetto tone . . . 0·13205 from each end and in the middle.

The second falsetto tone . . $\left.\begin{array}{c} 0·09435 \\ 0·35535 \end{array}\right\}$ from each end.

When a cord, as commonly happens, is stretched between two fixed points and struck, it takes between these two points a curved position, and consequently, a greater length with necessarily a greater tension. The influence of this greater tension must become greater in proportion to the extent which the cords have for vibrating in; and, hence, it follows, that the tone of the cord when it is strong, must be higher than when it is weak. This difference is particularly remarkable, when the tension of the cord is not great. In most stringed instruments, however, it is not observable to the ear. Should, says Weber, more instruments be constructed in future, after the manner of the mercantile harmonichord, where each tone swells and decreases by itself; so it will lead to dissonance, when no compensation is allowed for greater tension combined with greater vibration. This compensation, is, therefore, produced by bending the cord over two bridges, in such a manner, that it may pass over the one and under the other. The angle of these bridges is not sharp but round, and should the cord be struck, so that the vibration happens in one of the bridges' vertical planes, so by great vibrations,

* Jahresbericht, 1834. Poggendorff's Ann. xxviii. 1.

a portion of the cord will unfold itself upon the bridges, the vibrating cord will thus be lengthened and the tension diminished. By this means, the duration of the vibration will be increased which had been lessened by the greater tension; it becomes no difficult calculation, to find the proportion at which both influences compensate themselves, and at which this practical correction is available.

The experiments of Weber and Hällström upon compound tones did not agree, but by new researches, the former has ascertained, that one and the same cord (without reference to the falsetto tone) gives not merely one fundamental tone (grundton) but two, and, perhaps more, but which cannot be discriminated. When these tones are produced at the same time, a sound which is marked as false affects the ear. Wherefore this, which was not before taken into account by the theory, has its foundation in the circumstance, that, in the theory, the cords are considered as very flexible fibrous bodies, as bodies which do not occur in nature, while yet they may be so fine and so long, especially when they are made of metal, as to be considered elastic rods. Weber made some trials with fine and large brass, and iron wire, and found the deviations to increase with the thickness of the cord and hardness of the metal. Since, by calculating these results, we find a certain agreement with natural phenomena, Weber thinks, that a more rigid theoretical calculation would explain the variations in the tones obtained by Baron Blein, and also in the organ pipes described in Hällström's experiments.

2. Strehlke* has published a mathematical paper upon the situation of the nodes of vibration in straight elastic rods, which vibrate transversely when both ends are free. The object in view, is the same as that obtained by Bernoulli and Ricatti. The calculations are accompanied by experiments which shew their agreement with theory.

3. M. Cagniard Letour† has found, 1st, that the tone produced by the longitudinal vibration of a metallic rod becomes neither higher nor deeper, by tempering the rods with hammers. 2nd, A steel-wire tempered by sudden cooling, gives, in longitudinal vibrations, a deeper tone

* Poggendorff's Annalen, xxvii. 515; xxix. 512.

† Journal de Chim. Med. ix. 309.

than when not tempered. 3rd, A tuning fork hammered hard gives a longer tone than one heated. 4th, The rapidity of sound appears to be equal in ice and water, at 32°. The experiments of the same author upon the vibration of liquids have been already noticed.*

4. Pellisor has endeavoured to form a theory of acoustical instruments.† He considers that the sound consists of the vibration of the molecules, or smallest portions of the sounding body, and not, as Chladni and Weber consider, in the total vibration. He brings forward the following experiment to support his position. If we sound one of the strings of an instrument by pulling it in the middle with the fingers, it soon ceases to sound, notwithstanding, it makes vibrations a line broad; but on the other hand, the tone of the string, when sounded by a tangent force, is very strong, while the vibrations have scarce an appreciable width.

5. *Method of tuning a Guitar without the assistance of the ear.*‡ This method, proposed by M. Bary, professor of Physics, at the Royal College of Charlemagne, in Paris, depends on the circumstance, that the communication of vibratory sounds is most effective through elastic media, when the bodies in the vicinity of the original vibrated body are capable of vibrating in unison with them. When, therefore, two strings fastened near each other possess, for their concord, the necessary tension and length, and one of them is made to sound, the vibrations are, with much force, transferred to the other, and this transference can be made, as Saveur has shown, perceptible to the eye, by placing a saddle of paper upon the string, at first, in a state of rest. When this string *hears* the other, the saddle will be shaken, and fall off. When both strings are in harmony, the paper will be very little, or not at all shaken.

6. *Effect of Sound on the Barometer.*—Sir H. Englefield, while at Brussels, in 1773, made some experiments on this subject. The barometer was fixed in the opening of a window, in the north-east tower of the church of St. Gudule, about 7 feet from the summit of the bell. Mr. Pigott found the height of the barometer 29·478 inches. It did not vary

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* Records of General Science, vol. i. 98.

† Jahrbuck der Chemie und Physik, vii. and viii.

‡ Poggendorff's Annalen, xxxv. 524.

until the clapper was loosened, when the mercury rose and continued to undergo a kind of starting, every time that the clapper struck the bell. Mr. Pigott observed the height of the mercury, during the sounding, 29·469. Sir H. Englefield found its maximum height 29·480, minimum 29·474; maximum 29·482, minimum 29.472. Hence, the effect of sound upon the barometer extends to the $\frac{1}{6000}$ and $\frac{1}{16000}$ of an inch.* It is remarkable, that Pigott generally made the height $\frac{1}{5000}$ less than Englefield. The latter attributes such discordances to the difference in the eyesight.

<center>II. OPTICS.</center>

1. *Stroboscope.*—Stampfer has invented some interesting stroboscopical tables, or glasses, founded upon a similar principle with the traumatoscopical figures. The first idea of this instrument originated with Plateau. He termed it phenakisticope.† By turning a wheel, figures are seen to walk, jump, pump water, &c. The table, or plate, is circular, and moves round on its centre. The actions consist of 8 or 10 postures. If it is wished to represent a man bowing, the first position is, a man standing straight; in the second, he has a slight inclination; in the third, still more; and so on to the sixth position, where he has the greatest flexure; the four following represent the figure recovering its straight posture, so that the fifth and seventh, the fourth and eighth, the third and ninth, the second and tenth figures have the same posture. Between each of the figures on the periphery of the plate, there is a slit ¼ inch long, and ¼ inch wide, in a direction parallel with the radii of the plate, and extending to an equal distance from the centre. If the image is placed before a mirror, and the plate is made to swing round on its centre, while we look through the apertures held before the eye, we shall observe in the mirror, the figures bowing continually, and with a rapidity proportionate to the rate at which the plate turns round. The illusion depends on the circumstance, that the plate between each aperture is covered, while the figure goes farther. That the deception may be complete, it is

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* Young's Lectures, ii. 269.

† Correspond. Math. et Phys. de l'Observatorie de Bruxelles, June, 1833. Jahresbericht, 1834, 22.

necessary, that every part of the figures which is not bow-ing shall be at an absolutely equal distance from the centre of the plate, and from the opening in the periphery, and also, that the figures possess equal thickness and colour.

2. *Photometer of Maistre.**—This instrument consists of two equal prisms laid upon each other, so as to form an even plate. One prism is formed of dark blue glass, and where the acute angle lies upon the base of the white prism, the diameter of the blue glass is small, and then constantly increases to the point where the edge of the white prism lies upon the base of the blue. By a compari-son of unequal parts of the prism, where the light ter-minates, a comparison is obtained between the different intensities of light. Quetelet has formed a photometer upon a similar principle.

3. *Influence of Colour on the radiation of non-luminous heat.* —In 1833, Dr. Stark published an experimental inquiry on the alleged influence of colour on the radiation of non-luminous heat.† Mr. Powell, of Oxford, soon after objected to these experiments,‡ considering them inapplicable. Professors Bache and Courtenay, of the University of Pennsylvania,§ have, since the publication of Dr. Stark's paper, taken up the investigation, and confirm the objec-tions, in some measure, of Mr. Powell, especially, in re-ference to the experiments upon the absorption of radiant heat, as tested by the inverse of Count Rumford's method for comparing the conducting powers of substances used for clothing; also, as tested by the effect of the heat from the flame of an argand gas burner, thrown by a mirror upon the bulb of an air thermometer which was variously coated. Of the same class, also, were the experiments on radiation as tested by the method used by Count Rumford already mentioned; the enveloping materials of the inner thermometer being wools of different colours, and coloured wheat paste. An unexceptionable mode in the view of Mr. Bache, which was adopted by Dr. Stark, was that of filling a glass globe with hot water, and covering it with different pigments. Mr. Powell disapproved of this method, unless the radiating covering of the globe were equalized in re-

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* Jahresbericht, vi. 34.   † Philos. Transact., 1833.

‡ Edinburgh Journal.   § Journal of the Franklin Institute, November, 1835.

spect to thickness, conducting power, density, &c., and referred to the experiments of Sir John Leslie, in which, equal quantities of different radiating substances were dissolved, and spread upon a surface for comparison. Mr. Bache answers, " that equal thicknesses of substances, possessing different radiating powers, should be compared together, seems to me, disproved by the law established by Sir John Leslie's own experiments, viz., that radiation takes place, not only from the surface, but in a thickness, which is appreciated in good radiators. Thus, when different coatings of jelly were applied in succession upon one of the sides of the cube, in Professor Leslie's experiments, the radiation increased with the thickness up to a certain point. The effect of the conducting power appears, by the same experiment, to be so small, that an increase of the thickness in the bad conductor was actually more than compensated for, by the increased radiating power. The influence of density on conducting power is well known; but the effect of either, as controlling the radiating power of a substance, or as modifying it, is, I apprehend, yet to be appreciated."

Mr. Bache employed, in his experiments, tin cylinders 2 inches high, and 1½ in diameter, closed at the bottom with a slightly conical tube fitted to the top, to receive a perforated cork, through which the stem of a thermometer was admitted. The colouring matter, whose influence on the radiation was to be determined, was applied to the cylinder, which was filled with hot water, and the time of cooling noted. The results which he obtained are decidedly unfavourable to the specific effect of colour in determining the radiating powers of bodies. In one set of experiments exhibited in a table, blue is above black in the beginning of the table, and occurs again at the 18th place. Although the first seven numbers were blue or black, the 9th, 10th, 11th, and 12th were white, black, blue and white respectively. White is in the greater number of cases in the middle part of the table, ranging close to black. Hence, it is concluded, that the alleged advantages of dark clothing, during cold weather, has been too hastily inferred; and that, if a person is not exposed to the sun, the particular colour of the clothing is not of real con-

sequence. Neither does roughness appear to be a determining quality, for, though generally, the smooth surfaces are lower on the list, this is not universal. The rough sulphate of barytes is lower on the list than the smooth carbonate of lead. Plumbago occupies a low place, and Indian ink a comparatively high one. The best radiators do not appear to belong to any particular class of bodies; litmus paper and Prussian blue are next each other, while sulphuret of lead, and bi-sulphuret of tin are separated by an interval of 15 bodies. We have no doubt, that this subject will attract more attention in this country, as it is fraught with considerable practical utility.

4.*Effect of Light in magnetizing Needles.*—Mr.Draper* has repeated the experiment of Mrs. Somerville, which consisted in rendering a needle magnetic by placing it under a piece of glass, or blue ribbon, having half its length protected by paper. He did not succeed. He made a very delicate experiment, by admitting "a divergent beam of light through a hole in the shutter of a dark room; the cone of luminous matter, at its apex, was about $\frac{1}{15}$th of an inch in diameter, and a hair, or other filament held in it, exhibited the phenomena of diffraction; the colours being received into the eye by a lens. Across this beam a silver wire was adjusted, and each of its extremities connected with cups of mercury, which communicated with the poles of a voltaic battery. It was expected that, if there was any action between a magnetic filament and light, some derangement would be seen in the diffracted fringes, when the current passed; but none such was observable." He found also, that solar light concentrated upon a delicate needle, produced no effect, either in the air or in vacuum. " A needle made of watch spring, about 4 inches long, which in an exhausted receiver, suspended by a filament of silk, exhibited no polarity, had one half of it exposed to the violet ray, cast by an equiangular prism of flint glass. This ray was separated from the others, by passing it through a slit in a metallic screen, and half the needle shielded by a piece of paper. After two hours exposure, it was suspended again in the exhausted receiver, but still showed no token of polarity; it was then exposed to the other rays successively, with the same result."

* Journal of the Franklin Institute, February, 1835.

Mr. Boyle found, that a piece of amber would become electrified by exposure to a sunbeam. Mr. Draper produced the same effect on ruby from Ceylon, rolled sapphire, a tourmaline, a Brazilian emerald, a topaz, and likewise glass. He attributes this to the agency of the light, and not to the heat; because, when exposed to the action of heat from another source, in the same degree, no such consequence followed.

5. *Absorption of light.*—The remarkable phenomena discovered by Sir David Brewster, of absorption in light, which has passed through certain coloured gases have been examined by Wrede.* According to him, one part of the light is retarded relatively to the other part, in a quantity proportionate to the nature of the body; and which, consequently, must be different in different bodies. If this hypothesis is admitted, then the phenomena discovered by Brewster follow, as a necessary consequence. Without presuming to explain in what way this retardation takes place, Wrede has calculated its effects on three different suppositions; 1. on the supposition of a simple retardation; 2. on the supposition of an infinity of partial reflexions, between the particles of matter analogous to those which take place between the two plain surfaces of a translucid body; and, 3. on the same supposition extended to a great number of particles, that is to say, supposing the influence of bodies upon light, will be the same as that of a great number of plain and equi-distant surfaces. M. Wrede has constructed the formulæ representing the resultant intensity, by taking for the abscissa of the constructed curve, the logarithms of the proportions between the retardation and the lengths of the undulation, and for the ordinates the corresponding intensities. The resulting curve resembles somewhat a spiral wire.

The difference between the logarithms of the two numbers, being independent of the absolute values of these numbers, the distance between two points of the axis of the abscissa, which correspond to the limits of the spectrum, ought to be the same, whatever the extent of the retardation is; it is clear, that this distance ought to be equal to the logarithm of the proportion between the

* Bibliotheque Universelle, June, 1835.

length of the undulation of the extreme red and extreme violet.

The phenomena of absorption which take place in bromine gas and in potash-oxalate of chromium, observed by Brewster, are explained in the same manner; and, the differences which exist between these phenomena, only proceed from differences between the absolute values of the retardations. In order to explain the phenomena noticed in nitrous acid and euchlorine, it is necessary to admit more than two retardations. This difference is not remarkable, because the gases are compounds. The spectra derived from certain coloured flames, are explained in the same manner as the phenomena of absorption. But, besides the supposition of retardations, it is necessary to admit, that certain flames give only certain kinds of light. Some of the phenomena of this kind of spectra proceed only from such a cause; such is the case, for example, in the orange tint observed in the spectrum, proceeding from the flame of a candle. Wrede has proved, that this tint, proceeds from the circumstance, that the exterior part of the flame, where true combustion takes place, affords an absolutely homogeneous light, and, consequently, undergoes no dispersion.

III. ELECTRICITY AND MAGNETISM.

1. *Method of determining the electrical conductibility of small masses.*—The usual method of determining this property in bodies, consists in interposing between an electrical source and a metallic wire attached to a sensible electroscope, the body whose conductibility is to be ascertained. For this purpose, an electrical machine, a voltaic or a dry pile is employed. Several ingenious apparatus have also been substituted. Lassaigne recommends a modification which he has found to answer. To one of the wires of Schweigger's multiplier, he attaches a small platinum spoon, containing dilute nitric acid; above this spoon, is fixed upon a support, a small glass tube, 2·3 inches long, and ·19 inch in diameter. A wire of red copper curved at one of its extremities, traverses it for two-thirds of its length. To this distance the wire is flattened into a spatula, or terminated by a disk. To this part of the wire the body to

be tried is attached.   It is then touched on the other side
with the end of the other wire of the multiplier, and then
the curved portion of the copper wire is plunged into the
nitric acid.   If the body placed between the two wires is a
conductor of electricity, the magnetic needle instantly
deviates.   He has also found, that a thermo-electric cy-
linder is very convenient : it is formed, by soldering, end to
end, two small cylinders, the one of Bismuth, and the other
of Antimony.   When placed in a glass tube and slightly
heated at the point of union, it was placed in contact on
one side, with one of the wires of a multiplier, and on the
other, with the substance to be tried, and touched at its
opposite extremity, with the other wire of the multiplier.
The results were similar to those obtained by the first
method; *Arsenic* and *Tellurium* were found to be conductors.[*]

2. *Chemical action of electrical currents.*—The experiments
of M. Botto, lead to the conclusion, that the direction of a
magneto-electric current, has an influence like that of a
hydro-electric current upon the facility which it may have
in passing through the same system of conductors.   Mr.
Faraday has proved, that the different substances which
form a circle, experience in similar circumstances, an equal
magneto-electric induction, and, consequently, a tendency
to produce the same current.   Botto has confirmed this
fact.   He disposed a magneto-electric helix, having two
distinct and equal ends, in such a manner, that when it
was traversed in a contrary direction by two currents deve-
loped by influence, these two currents neutralized them-
selves.   If in the circle, which these currents are obliged
to traverse, we place a vessel filled with acidulated water,
and communicating with the conductors on one side by a
wire, on the other side by a plate of the same metal, the
currents are neutralized.   But, if one of them is made
stronger than the other, by a change in the number of the
spirals in the magneto-electric helix, the effect upon the
galvanometer which results from this difference of inten-
sity, is much more decided, when the most powerful cur-
rent passes into the liquid from the wire to the place, than
in the contrary direction.   Hence, it would appear, that
we are to attribute the double phenomenon which the same

[*] Journ. de Chim. Medic. i. 630.

heterogeneous circle presents, under the relations of electric conductibility, to the difference of chemical re-action which accompanies the passage of the currents.*

3.*Employment of electricity in dissolving calculi.*—M.Bonnet, Surgeon of the Hotel Dieu, at Lyons, proposes to dissolve stones in the bladder, by injecting a solution of nitrate of potash into the bladder, and then introducing electrical conductors on each side of the calculus ; the electrical influence being exerted, will evolve nitric acid on one side, and potash on the other. So that whatever be the composition of the calculus, if soluble in an acid or alkali, it must be acted on. When the calculus consists of plates, as in urate of ammonia and in triple phosphates, or if it is porous as in ammoniaco-magnesian phosphates, he found, that it was softened, and that its layers readily separated. A great objection to this ingenious method is its extreme tardiness. Oxalate of lime acted on by a pile of 100 pairs of 2½ inch square plates was not attacked in the course of a night ; the others were partially destroyed.†

4.*Electricity developed by the friction of metals.*—Becquerel discovered, that by rubbing one metal against another, placed at the two extremities of a galvanometer, an electric current is produced, in which, one of the metals exposed to friction is positive, and the other negative. He also shewed, that in causing some metallic dust to slide over the surface of a metal, of the same, or of a different nature, opposite electric tensions were produced in the metal, and in the dust.

M. De la Rive has prosecuted the subject, and has found, that the slightest friction with the finger, or with any substance, is sufficient to determine on a metallic surface a tension often remarkable. The simplest mode of performing the experiment, is to bring in contact with the plate of the condenser, pieces of metal of different kinds, held by means of isolating handles, and then to rub gently the surface of these metals with the finger, which ought to be very dry. Employing as agents for friction, the finger, ivory, horn, cork, and other species of wood, he found, that the following metals acquired by friction negative electricity :

* Bibliotheqne Universelle, February, 1835.
† Ibid. April, 1835, 391.

Rhodium, Platinum, Palladium, Gold, Tellurium, Cobalt, and Nickel. Silver, Copper, Brass, and Tin, are also almost always negative, but sometimes positive. *Antimony* has once or twice given positive signs. The nature of the electricity developed in *Iron and Zinc* was variable, although in iron, the tendency was to a negative state. *Lead and Bismuth* were constantly positive, the latter highly so.

The circumstances under which the two electricities are developed, in respect to the uncertain metals as they may be termed, are as follow :—in very dry air, and with the fingers or wood well dried, these metals are always negative, as the surface may be well polished or more or less oxidated. When the surface rubbed is very great, and when the rubbing body is drawn along all its extent, the metal becomes positive. It becomes more readily positive, when rubbed with cork than with wood, in the same circumstances. The electric effect is increased by an elevation of temperature. Iron, Zinc, and Tin, exposed for some moments to a high temperature, and rubbed immediately after upon a very smooth face, gave most frequently positive electricity, while, if in the same circumstances, the friction is performed over an angle the indications are negative.

De la Rive considers, that all the metals when polished and rubbed take negative electricity, but that the more oxidizable metals, in consequence of their possessing a thin imperceptible layer of oxide on their surfaces, develope under these circumstances, positive electricity.

This source of electricity should be carefully attended to in experimenting with the metals. De La Rive states his conviction, that an electrical effect has often been ascribed to contact, which was in reality to be attributed to friction. Thus, he considers it probable, that the electrical signs obtained in the experiment, when two insulated disks of copper and zinc are alternately brought in contact and separated, are owing to the friction of one metal upon the other. He has produced, by friction in the method described, an electrical current, but has not obtained any indications of tension, because the two substances rubbed are such good conductors, that the two electricities re-unite immediately after their separation.[*]

[*] Mem. de la Soc. de Phys. et d'Histoire Nat de Genève, vi. 174.

5. *Atmospherical Electricity.*—M. Matteuci has lately made some interesting experiments upon this subject. They were conducted, in what is termed in Italy, an English wood (that is, one of small extent) consisting of *Robinia pseudacacia, Platinus Occidentalis, Gleditzia triacanthos, Melia, &c.* The electroscope with which the experiments were made consisted of a stem of wood, at the extremity of which, was placed a common lamp; a copper wire conducted the electricity from the flame to an electroscope. On rainy or windy days, a very thin portion of phosphorus was substituted for the lamp, and was kept in a tube of glass terminating in a point. He found, that whenever the electricity of the atmosphere is positive, (which is always the case in calm weather), it is impossible to have any traces of electricity in the interior of a wood. The most curious mode of observing it, is to move, carrying the electroscope in the hand, either out of the wood, or above the leaves. The flame is scarcely removed 10 paces from the trees, when traces of electricity begin to appear. These increase with the distance. In returning, the first tree is scarcely reached, when the electroscope ceases immediately to indicate the presence of electricity. These general results can only be explained by one of two hypotheses; either, that the electricity of the air is discharged by the leaves and the vapour of water, and escapes by this means into the earth, or, that there is developed by the effect of vegetable life,—by the respiration of plants, enough of negative to neutralize the positive electricity of the surrounding air. The second hypothesis appears most plausible, because it is difficult to admit the second, when we attend to the conducting power of the flame, and of the column of hot air which is much superior to that of the leaves.

The results of a great number of observations showed, that in the night, signs of electricity are often absent, both in the air, and in the interior of a wood. At the approach of day, before the sun appears above the horizon, decided indications of negative electricity appear among the trees, while none are detected in the open air. We can readily understand this observation, if we admit that oxygen is disengaged from the leaves before the rays of the sun strike them directly. In this case, negative electricity appears.

If the sky is calm, the signs of negative electricity disappear in the interior of the wood, at the same time that positive electricity is developed in the air. On three days, when the sky was cloudy, and almost stormy, negative electricity was detected in the external air, and in the wood. Hence, it may be inferred, that negative electricity is disengaged by vegetation during the day, which is constantly neutralized by positive electricity. Matteuci has promised to continue his observations, and expresses a strong desire that similar investigations should be undertaken by meteorologists in other parts of the world, especially in reference to rain.*

6. Nobili's paper, *on the distribution and effects of electrical currents in conducting masses,*† derives much interest from the circumstance, that it constitutes the last literary work of this active philosopher, who died in August last, aged 51, having been born in 1784. In this paper, he sets out with investigating the distribution of currents, in metallic conductors, in liquid conductors, and at the points where the currents pass from one conductor to another. He describes two modes of studying electric currents; 1st, By the galvanometer. This process consists in procuring a small tube equally large and deep in all its extent, and from one to two feet in length. It is placed horizontally, and closed at each of its extremities by means of two metallic plates of similiar dimensions to that of its orifices. The plates are intended to introduce the electric current into the canal, after the latter has been filled with mercury. After the current has been introduced, the points of the *galvanometric explorer*‡ are brought to the surface of the mercury. The galvanometer indicates a certain deviation, 12° for example. If the points are made to penetrate into the interior of the mass, the deviation remains stationary. Such is the method applicable to the first class of uniform conductors. For conductors of the second class, we must substitute acidulated, or saline water, for the mercury.

* Bibliotheque Universelle, May, 1835, 38.

† Ib. July and August, 1835, 263, 416.

‡ This consists of two similar platinum points fixed in a piece of wood or cork, in order that they may be kept at a proper distance from each other, and each communicating with one of the extremities of a sensible galvanometer.

Currents passed through this liquid affect the galvanometer equally at all depths. In investigating the passage of currents from the first class of conductors to the second, that is, from metals to liquids, it is necessary to make use of, 2nd, The electro-chemical method. Here the tube is divided into two equal compartments, by means of a platinum plate, placed transversely so as to prevent any direct communication between the liquids placed on each side of the plate. Both compartments are filled with a solution of acetate of lead, and the current is passed through them. The metallic diaphragm is observed to be coloured on one of its faces with the electro-positive elements of the solution; while the other surface receives the lead which covers it with a thin layer of fine powder. The decomposition takes place equally over the whole metallic surface, which demonstrates the uniformity of energy in the current, in every part of the conductor. The same appearance takes place on the negative surface.

1st. The results obtained for uniform conductors are, that currents possess the same electro-dynamic force equally in all parts of the mass which they traverse, and the same chemical power at each of the particular points, where the current passes from the metal into the liquid, or from the liquid into the metal.

2nd. In using conductors which were not uniform in all their extent, it was found, that in order to double the chemical effect, it is not sufficient to employ a current possessing a double electro-dynamic force; it is necessary to use a stronger proportion.

3rd. In support of the results obtained by De la Rive, Nobili found, that currents undergo great difficulty in passing from a liquid into a metallic conductor, and *vice versa*. This difficulty is so considerable, that a considerable portion of the currents prefer circulating round the diaphragm rather than penetrating it.

4th. When we introduce two platinum plates into a liquid, as in common decomposition, we can distinguish in the deposit, upon its surface, three degrees of thickness, *strong* on the edges of the plate, *moderate* on the central parts of the anterior face, and *weak* on the central parts of the posterior face. We have seen, that where the metallic

plate presents a surface equal to that of the liquid, the current proceeds from all parts of the surface with an equal force; but in the present instance, this does not happen, and *the electricity directs itself to the point where it can most readily discharge itself.* The facility in discharging depends on the conductibility of the bodies which receive the discharges and electric currents; and in general, bodies conduct well in proportion as they are short and thick. In the first case mentioned, where the conducting liquid possesses uniform dimensions, we find that the current passes from every part of the plate with an equal intensity, because every part has a corresponding liquid stream, conducting equally in relation to mass and length. When the canal is narrower in the middle, this contraction carries off from the lateral streams of fluid a part of their mass, and renders their conducting power inferior. The contrary takes place, when the canal is expanded in the middle; a new mass is added to the lateral streams of the liquid, and increases their conductibility. In the case of the two plates mentioned at the beginning of the paragraph, the streams of the surrounding liquid mass unite with those which establish the direct communication between the edges of the two plates, and thus present a great number of additional conductors for the electricity which arrives at these edges,—conductors which are entirely absent in the central parts.

5th. If two piles, of equal tension, are charged with different strengths of liquid, the one to produce a current of $40°$, and the other of $5°$, the strongest current will divide into two streams, one of which, will circulate by itself, and the other will unite with the feeblest current, and circulate with it. When the poles of two piles, of equal tension, are placed parallel, but in an opposite direction, there appear to be three currents circulating, one for each pile, and a third common to both. When the poles are placed parallel, and in the same direction, the two piles are discharged independently; and hence, it appears that two piles cannot make part of the same circuit, unless the currents are obliged to cross each other in the conducting liquid.

Nobili terminates his paper with some theoretical views in reference to the efficacy of doubling the surface of the

copper in Wollaston's pile, and on the influence of surfaces, and irregularities in the pile.

7. *Paralysis of the Tongue treated by Galvanism.*—Jules Roula, a patient of M. Palafrat, was seized with apoplexy, and for thirteen years subsequently, that portion of the nerves of the 9th pair which serve for articulation, was paralysed. Palafrat began by treating him by acupuncture in the nape of the neck, in the direction of the base of the brain. The needle was made to communicate with the negative pole of a strong voltaic pile; a plate of platinum, enveloped with a rag soaked in saline water, was placed on the tongue, and communicated with the positive pole of the pile. The currents were interrupted and regulated by a watch. An insupportable metallic taste was first produced; violent contractions of the tongue and stomach followed; ultimately, vomiting was almost produced; and, then he exclaimed, throwing from him the apparatus, " *Je parle, merci, Monsieur le medecin; Je parle, merci.*" He then repeated several sentences, but could not pronouuce *j* nor *r*. The same treatment repeated five times, rendered the patient capable of articulating these letters. The treatment was begun on the 27th of November last; and on the 22nd of December, when the patient was presented to the academy, he could repeat several sentences very intelligibly, but had always a tendency to become confused.

8. *New method of magnetizing.*—M. Aime recommends the following method, which consists in tempering and magnetizing a bar of iron at the same time. To effect this, a bar of soft iron curved in the form of a horse-shoe, is surrounded with a brass wire, covered with silk; the two extremities of this wire are made to communicate with the poles of the voltaic pile; a bar of steel equal in length to the distance between the two extremities of the horse-shoe is then ignited, and seized between a pair of pincers; the two poles of the horse-shoe are then applied to the bar, and plunged into a bucket of water; in the course of a minute or two after immersion, the bar is detached from the horse-shoe, and a similar operation performed with similar bars extracted from the fire. In order to prevent the brass wire from softening, care must be taken in dipping the apparatus in water to envelope the two extremities of the

helix in a rag covered with mastic. The ends of the conducting wire were soldered to the zinc and copper poles of the battery; a single wire was employed. Aimé, however, considers that it may be preferable to unite several into a bundle, or even to take a ribbon of copper covered with silk or varnish. The bar ought not to be detached too quickly from the horse-shoe; it is necessary to wait until the interior of the steel has acquired a slight elevation of temperature, in order that the molecules may have time to arrange themselves, conveniently, for magnetizing and tempering. The duration of the immersion varies with the size of the bar, and the temperature which it possesses when taken from the fire.[*]

9. *Magnetism by common electricity.*[†]—M. Llambias has addressed a manuscript upon this subject to the French academy. The results of his experiments were, 1. In every metallic conductor traversed by the discharge of a Leyden phial, two *magneto-electric* currents are simultaneously discharged, which pass in opposite directions, one of which may be said to proceed from the vitreous to the resinous pole, and the other from the resinous to the vitreous pole. 2. The currents can be partly separated from each other. This separation may be effected in dividing a discharge between two or several different branches of the same circle, when in some particular branch there is an interruption which gives origin to the spark. 3. This separation of currents is more or less practicable, and is comprised within certain limits, which can be nearly determined by experiments for each discharge, and for each of the other elements which produce the phenomenon. 4. The separation of these currents may take place in any portion of the circle submitted to the discharge, at the same time that the other parts of the same circle are traversed by currents completely re-united. 5. In every circle, or every portion of the circle, which the two currents traverse in union, it is, in general, the current which passes from the vitreous to the resinous pole, or the primitive current which has the chief effect in communicating the magnetic influence. 6. Each of the currents magnetizes so much the more strongly in proportion, as it is separated or disengaged from the

[*] Journal de Chimie. Medic. i. 370.   [†] Ibid, i. 36.

other; and, in general, we may say, that the magnetic power, produced by a discharge of the Leyden jar, is only the effect determined by the simultaneous union of two magnetizing, more or less unequal and opposed, forces. 7. The common simple spark of the machine produces analogous phenomena.

IV. HEAT.

Reflexion of radiating heat.—The researches of Leslie and Rumford have shewn, that rays of heat are reflected by bodies, according as the surfaces are more or less polished. But a natural question now presents itself, viz. What, in each case, is the proportion between the quantity of reflected and incident heat? The results obtained by Melloni on the immediate transmission of radiating heat, through many solid and liquid substances, afford a resolution of this question.* When calorific rays fall perpendicularly on the anterior surface of a diathermanous plate, possessing parallel faces, they undergo a certain reflexion, penetrate into the anterior, are partly absorbed, arrive at the second surface, are there again reflected, and pass out again into the air, pursuing their first direction. But in certain cases, there is no continual absorption, and where, consequently, the difference between the quantity of incident heat and the quantity transmitted is exactly equal to the value of the reflexions produced upon the two surfaces of the plate. Rock salt affords an excellent example of this. Plates of this substance, in a pure state, and well polished, transmit 0·923 of the incident heat, whatever be their thickness, and the nature of the rays of heat, or the modifications which these rays may have undergone, in their passage through other plates. Let us suppose two plates of rock salt, the one ·0154 inch in thickness, the other ·154. Then, from what has been said, it is obvious, that the transmission of the first plate will be equal to that of the second; and, if we suppose the first of these plates divided into 10 layers, each ·0154 inch in thickness, the absorbing power of the nine posterior layers (each ·0154 inch) will have no appreciable effect. Hence, if the rays undergo any absorption, it must take place in their passage through the first layer. Let us

* Institut. No. 130.

suppose that this takes place. Then the molecules, which form the first layer of ·0154 thickness, will form a *kind of sieve*, retaining all that is not completely transmissible by the rock salt; and the quantity of heat lost in the transit by one or other plate, that is to say, $1 - 0·923$, or ·077 will be the sum of the rays absorbed or retained, and of the rays reflected, to the two surfaces. When, therefore, radiating heat is received upon one of the plates, the thinnest for example, and when they are transmitted by the other, the supposed absorption, or *epuration*, will take place in the first, and no more will arrive at the second than the rays completely transmissible by the substance of which it is composed, with the exception of the quantity lost in the two reflexions; so that the loss experienced by these rays, in passing through the second plate, must necessarily be less than ·077. But experiment shows that on this passage, there is exactly 0·923 of heat transmitted, and ·077 of heat lost. Hence, no absorption has in reality taken place in the first transmission, and the quantity ·077 expresses only the loss produced by the reflexion of the radiating heat to the first and second surfaces of each plate. We can readily determine the special value of either of two reflexions. If we term R the reflexion for unity of incident heat, then $1 - R$ will be the quantity which will penetrate into the interior of the plate, and $R(1 - R)$ the reflexion which the latter will undergo on the posterior surface; for, as the salt possesses no absorbing power, the whole quantity $1 - R$ arrives at the second surface, and is reflected in the proportion of $R : 1$. Now, the sum of the two reflexions, added to the quantity transmitted, 0·923, ought to produce the quantity of incident heat, which we suppose equal to unity.

We have then the equation,

$$R + R(1 - R) + 0·923 = 1.$$

from which we deduce,

$$R = 1 + \sqrt{0·923} = 1 + 0·9607.$$

The first sign of the radicle, as it leads to an absurd result, should be rejected. The reflexion at the anterior surface of the plate will then be $1 - 0·9607 = 0·0393$; and such will also be the proportion of the second reflexion, relatively to the quantity of heat, which arrives at the

posterior surface of the rock salt; but if we wish to have the absolute value of this last reflexion, we shall obtain it by substituting ·0393 in place of R in the expression R (1 — R), or more simply, by taking the difference between the numbers ·077 and ·0393, which gives, in both cases, ·0377. Experiment afforded similar results with glass, rock crystal, alum, fluor spar, topaz, sulphate of barytes, &c. Melloni concludes, that we may say, as a general expression, that radiant heat experiences a reflexion of about $\frac{4}{100}$ths of the incident quantity, when it falls perpendicularly upon the surface of diathermanous substances. With regard to the heat reflected by athermanous bodies, it is necessary, first, to observe the effect of the transmission of heat through rock salt, when the radiation, derived from a constant source, is perpendicular to its faces. The plate is then inclined to the incident rays. No sensible diminution in the quantity of heat transmitted is exhibited as long as the inclination does not exceed 30° or 35°. The reflexion of the perpendicular rays is then sensibly equal to that which the rays forming an angle of 55° to 60°, with the reflector, undergo. If now, we throw upon the well polished surface of a very large plate of glass, or rock crystal, a compact quantity of radiant heat, at an incident angle of 55° or 60°, and receive the portion reflected in the interior of the tube, which surrounds the pile of the thermo-multiplier; having noted the force indicated by the galvanometer, and repeated the same experiment upon the polished surface of an athermanous body, we shall have a second force different from the first. The reflexion of the athermanous body will obviously be equal to the number ·0393 multiplied by the proportion of the two forces observed.

The following exhibits a comparison between rock crystal and yellow copper.

Reflexion of rock crystal, ... 3·15
Reflexion of yellow copper, . . . 35·63
Proportion of the two reflexions, . . 11·30
Product of the two numbers, } . . ·044
    ·0393 and 11·3 }

Article VI.

Chemical Analysis of Tabasheer. By Thomas Thomson, M.D., F. R. S., L. & E., &c., Regius Professor of Chemistry in the University of Glasgow.

Having lately received, from Calcutta, a very fine specimen of *tabasheer*, I was naturally induced to make a few experiments on its chemical constitution.

It is sufficiently known that tabasheer is a concretion met with occasionally in the joints of the bamboo; that it has been long employed in medicine, in Hindostan and the East; that it is very much esteemed; and, that it sells at a considerable price. The first good description of it was drawn up by Dr. Russel, and published in the " *Philosophical Transactions*," for 1790, p. 273. The specimen, laid before the Royal Society, by Dr. Russel, was put into the hands of Mr. Smithson for chemical examination. A very minute, accurate, and complete set of experiments, by this acute and accomplished philosopher, was published in the " *Philosophical Transactions*," for 1791, p. 368, from which it appeared, that the tabasheer was composed of silica nearly in a state of purity.

In the year 1806, a specimen of tabasheer, from Peru, was put into the hands of Fourcroy and Vauquelin, by Humboldt and Bonpland. These chemists subjected it to analysis, extracted from it 70 per cent. of silica, together with a little lime, and concluded (though it is not easy to see the evidence), that the tabasheer, which they examined, was a compound of 70 parts of silica, and 30 parts of potash. But under the potash were included the vegetable matter which' they showed it to contain, and also, the water, the amount of which, they seem not to have thought of determining.

In 1819, a curious paper on the optical properties of tabasheer, was published in the " *Philosophical Transactions*," by Dr. Brewster. An abstract of this paper, together with several particulars, relative to the history and formation of the tabasheer, was inserted in the eighth volume of Dr. Brewster's " *Journal of Science;*" and in the same volume, we have a chemical examination of the tabasheer, by Dr. Turner. This analysis agrees very nearly with that of Mr.

Smithson, and renders the accuracy of the statement of the great quantity of potash, announced by Fourcroy and Vauquelin, rather doubtful.

1. The tabasheer which I examined, was a very beautiful looking substance, in small irregular fragments of a blueish white colour and pearly lustre, not unlike chalcedony in appearance, but much softer. For it was incapable of scratching calcareous spar, and only slightly scratched sulphate of lime. When put into water, it gives out a great deal of air with a kind of crackling noise, and imbibes a great deal of water.

I found its specific gravity, (taken without allowing time for the internal air to escape), 1·9238. But, when by means of heat all the air bubbles had been driven off, the specific gravity was as high as 2·0824.

2. When ignited, it lost 4·87 per cent. of its weight. This loss consisted chiefly of water; but not entirely, for the tabasheer exhaled a peculiar odour, and, shewed evidently, the existence of a small quantity of vegetable matter in it.

3. Ten grains of tabasheer reduced to a fine powder were digested in distilled water for 24 hours. The water when concentrated was tasteless; but slightly reddened vegetable blues. Being evaporated to dryness, grayish scales remained, weighing 0·6 gr. These scales being digested in muriatic acid, a little iron was dissolved, but the scales consisted almost entirely of silica. Thus, it appears, that the silica in the tabasheer is still soluble in water. I am disposed to consider, the reddening of vegetable blues in this case, as produced by the dissolved silica; at least, I did not succeed in finding any trace of any other acid substance. When the muriatic acid dissolved upon the scales was evaporated to dryness, a brown matter remained, which besides iron, contained also a trace of vegetable matter; but too small to admit of examination. It contained also a little lime and a little silica.

4. Ten grains of tabasheer reduced to a fine powder, were mixed with 24 grains of finely pounded fluor spar, and the whole was made into a thin magma by means of sulphuric acid. This mixture was exposed for some hours to the heat of the sand bath in a platinum crucible. After the exhalations of fluosilicic acid had ceased, the crucible was

exposed to a heat gradually increased to redness, and kept
in that temperature till all the excess of sulphuric acid had
been driven off. The white matter in the crucible (chiefly
of lime) was now lixiviated with water, till every thing
soluble was taken up. The water thus employed, was mixed
with some carbonate of ammonia, and filtered to separate
the lime which it had dissolved in the state of sulphate.
The water, thus nearly freed from lime, was reduced to a
small quantity, by evaporation, and, while still hot, was
mixed with a few drops of solution of oxalate of ammonia,
to throw down a little lime which had either escaped the
action of the carbonate of ammonia, or had been afterwards
supplied by the filter. The mixture was allowed to stand
till it became clear, the liquid was then drawn off with a
sucker, evaporated to dryness, and the saline residue ex-
posed to a red heat. A salt remained, which weighed 0·2
grains, and which proved, on examination, to be sulphate
of potash, equivalent to 0·11 grain potash.

5. Ten grains of tabasheer in the state of a fine powder
were intimately mixed with 20 grains of anhydrous car-
bonate of soda, and the mixture exposed in a platinum
crucible to a red heat, raised at last sufficiently high to bring
the whole into a state fusion. The colour of the fused mass
was yellowish brown. It was dissolved in muriatic acid
the solution evaporated to dryness, and the residue, after
being digested a sufficient time in muriatic acid, was thrown
on the filter. The silica edulcorated, dried and ignited
weighed 9 grains.

6. The muriatic acid, in which the silica had been digested
being concentrated, was mixed with caustic ammonia. Yel-
low flocks fell, which were separated by decantation: these
flocks, when ignited, became dark brown, and weighed 0·1
grain; they dissolved readily in muriatic acid. The solu-
tion was super-saturated with caustic potash, and digested
on the sand bath for 24 hours. By this means 0·01 grain
of alumina was dissolved. The rest consisted of peroxide
of iron. Thus, the yellow flocks thrown down by caustic
ammonia consisted of Peroxide of iron, . 0·09
Alumina, ... 0·01
————
0·1

The liquid from which this precipitate had fallen was not

rendered muddy by carbonate of ammonia. It was, there-
fore, evaporated to dryness. A greyish matter remained
weighing 0·08 grain. This matter being digested in mu-
riatic acid, there remained undissolved 0·05 grain of silica.
The 0·03 grain dissolved, consisted of a mixture of alumina
and lime.

Thus, the constituents obtained were,

Moisture,	0·487	or	4·87
Silica,	9·050	„	90·50
Potash,	0·110	„	1·10
Peroxide of iron,	0·090	„	0·90
Alumina,	0·040	„	0·40
	9·777		97·77

The loss, amounting to 2·23 per cent., was probably the
consequence of my employing different portions of the
tabasheer in different steps of the analysis. For they were
not all exactly the same in appearance. Hence, possibly
the proportion of the constituents might vary somewhat in
each. But my supply of tabasheer was not sufficiently
great to admit of a new analysis upon a large scale. I did
not weigh the lime; but do not think it could exceed 0·1
per cent. It is needless to observe, that the preceding
analysis accords sufficiently with the experiments of Mr.
Smithson and Dr. Turner, and, therefore, serves to confirm
them. The tabasheer examined by Smithson, Turner, and
myself was from India; that subjected to examination by
Fourcroy and Vauquelin was from South America. It re-
mains to be seen whether the constitution of the American
tabasheer be essentially distinct from the Indian, as would
appear from the 30 per cent. of alkali, &c. found in it by
Fourcroy and Vauquelin.

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ARTICLE VII.

On Madder, and Madder Dyeing.

(Concluded from page 56.)

ON THE MODE OF DYEING COMMON MADDER-RED CLEARER
AND PURER.

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1. *Washing the madder.*— Numerous experiments have
proved that the colour is not improved by washing the

madder; but, that on the other hand, a great loss of colouring matter is experienced. The uniformity of the results of these experiments proves the error of the observation, that " by washing madder, clearer colours are produced." Even alizari, which of all the species of madder, gives up to water the greatest quantity of foreign matter, does not afford a clearer red after washing. It may be remarked, that madder, in the large way, should be washed with perfectly pure water, as water. containing. chalk renders a quantity of colouring matter effete.

2. *Addition of chalk in dyeing.*—The above-mentioned action of chalk upon the red of madder-red, demonstrates the advantage of this addition, which, by the new experiments of Schlumberger,* has received additional confirmation, that species of madder, which, by themselves give an easily deteriorating red, as Alsace madder, are thereby rendered durable.

3. *Addition of clay in dyeing.*—This is fully treated of in Runge's " *Farbenchemie.*"

ON THE SEPARATION OF THE CONSTITUENTS OF MADDER ON A LARGE SCALE.

The object of employing, in dyeing, the constituents of madder, instead of madder itself, is threefold, viz. to form a more beautiful, more certain, and a cheaper dye. A more beautiful and more certain effect is undoubtedly produced, when the three colouring matters are employed separately in their purest state, according to the method detailed; additions may be made to the colouring matters, as chalk and clay, to madder-red, and the choice of the mordants regulated, as the properties of the colouring matter render it necessary; as, for example, alum mordant with madder-purple forms *purple-red;* iron mordant forms *lilac violet;* and copper mordant with madder-orange produces *orange.* Runge has endeavoured to hit upon a method of separating these three colouring matters, so as to render them available to the manufacturer; but he has not succeeded in being able to render the price of the product proportionate to the price of the madder. The separation of the madder-purple from madder-red, especially, is accompanied with great diffi-

* Records of General Science, i. 208.

culty; and even, when the solution of alum in sulphuric acid is again used, there is still the expense of the indispensable treatment with spirit of wine.

The method of Robiquet of separating the colouring matter of madder, by sublimation, from the sulphuric acid and madder charcoal, would be the most preferable, as, by carbonizing the madder, the sulphuric acid employed may be again used in manufacturing salmiac, and in the formation of vitriol, and is thus restored. But, by this method, the dyes are not separated; for, by heating the madder-charcoal, both madder-purple and madder-red are sublimed together. Their separation must be effected by means of alum solution; and thus, a new difficulty arises. And, it is not to be overlooked, that the sublimation is attended with loss, as well as the subsequent treatment with alum and spirit of wine.

But, although this desirable object has not been attained, it cannot be doubted, that now, when the properties of the three dyes have been ascertained, a convenient method of separating them will soon be found out.

TESTING MADDER.

The goodness of any species of madder has hitherto been improperly attempted to be determined, by separating the colouring matter of madder when in solution; but the proper mode is, by its power of dyeing. This is shewn by the author in a table, where there are four sorts of madder differing in price from each other. They should all be employed in dyeing with completely pure water in the same manner. To show that this method gives a proper valuation of the goodness of a variety of madder, and that a quantitative estimate can be formed by means of the mordanted cotton, the varieties of madder are represented in four different degrees in relation to dyeing, from which it appears, that the darkness of the dye is in direct proportion to the quantity of colouring materials. Munjeet is the most powerful dyeing species of madder, next to it is the Spiers madder, then the Dutch, and lastly, the Avignon madder.

1. *Preparation of the mordanted cotton.*—In order to form a scale of colours, it is necessary to proportion the madder

and mordanted cotton to each other, and also, that the cloth used for the whole scale should be equally impregnated with the mordant, and dried, cleared, and again dried in the same manner. Therefore, the whole cloth must be impregnated with one and the same mordant, at the same time, dried by the same heat, equally washed, again dried by the same heat, and then divided. If this is omitted, irregular results are obtained, which proceed from the circumstance, that the cloth, by the absorption of the mordant, acquires weight, but unequally, according to the time it is allowed to hang in the air after dyeing. The different degrees of moisture of the cloth will give rise to considerable irregularity. The cloth must, therefore, be employed at one and the same temperature. Before placing it in the vat, it should be washed in a quantity of water, and then with distilled water. Pure, or distilled water must be employed in the dyeing.

2. *Quality of the Madder.*—When madder, especially that from Holland, comes in contact with the air, it absorbs moisture, and becomes darker. Avignon madder and munjeet take up much less. All examinations of madder are useless, therefore, unless it is exposed to a temperature of $212°$, and kept in glass vessels.

Summary.—The preceding details are briefly as follow :—

1. Madder contains three colouring matters.

2. These three colouring matters possess different properties, and, therefore, their separate use will be found of the greatest advantage.

3. Madder-purple does not admit of the addition of chalk, and gives, with the addition of clay, brighter dyes.

4. Madder-red does not admit very well of the addition of chalk and clay, but gives, with the assistance of these, clear and pure dyes.

5. Madder-orange is incompatible with both chalk and clay.

6. The oil mordanted cotton *(oelbeizkattun)* gives with the half, and even less colouring matter, as complete a colour, and even more complete than the common aluminous mordanted cotton *(thonbeizkattun)*, with the whole portion of colouring matter.

7. The aluminous mordanted cotton affords a very secure

means of determining the dyeing power, and relative value of the species of madder occurring in commerce.

Observations on Munjeet.—This species of madder is very rich in colouring matter, and the colour which it forms, with common aluminous mordanted cotton, approaches pure red. Hence, it is highly deserving of attention from the dyer and printer.

In commerce it occurs in two different species, 1. In bundles; and, 2. In powder. The munjeet in bundles consists of the thick and thin stalks of the plants, intermixed occasionally with small roots. The thin stalks are mostly covered with epidermis, and contain in proportion little colouring matter. The thick stalks, on the other hand, are bare, and two or three times richer in colouring matter than alizari, or Avignon madder. If 27 parts of aluminous mordanted cotton be dyed with 12 parts of the munjeet stalks, and a similar portion of cotton, with the same quantity of Avignon madder, the colour of the munjeet-red is twice as dark as that of the Avignon madder. The stalks of the munjeet are very dry, light, and porous; the fracture exhibits a number of small tubes which are empty. If then, 100 parts of munjeet stalks, cut into large pieces, are digested in cold water, and the colouring matter taken up by boiling with aluminous mordanted cotton, there remain after the evaporation of the fluid $7\frac{1}{2}$ per cent. of dry residue. Alizari treated in the same manner gives a residue destitute of dyeing properties, of $47\frac{1}{2}$ per cent. In the powdered munjeet, the rich thick stalks are mixed with the poor thin stalks.

A specimen of munjeet-red which was placed, during an equal time with one of Avignon madder-red, in a solution of 1 part chloride of lime in 3 of water, lost at least one half more of its colour. The same happened to two specimens which were formed with a double quantity of munjeet and Avignon madder *(Picard rouge palue)* upon oiled aluminous mordanted cotton.

According to Schwarz (Dingler's " *Polytechn. Journal,*" 1832, *Sept.* 385), the colour of munjeet-red upon oiled cotton after clearing is so fleeting, that in the light it is bleached in one day. This result agrees with the action by chloride of lime, which has much similarity with that

of light. Notwithstanding this action, the munjeet-red must be valued on account of its pure red colour, as its relation to soap, soda, and alum, shows. And, although it will not answer for producing Turkey-red, it may be employed with advantage for the formation of a clear red upon a white ground. It has the advantage over all kinds of madder, that it has no tendency to encroach upon the white ground; and this is still less the case, when clay is added in dyeing. The red, thus formed, is so clear, that the use of all purifying methods is saved. Munjeet gives, with iron mordant, shades which are very similar to those produced by madder-red. Munjeet, therefore, if the price is not too high, may be employed to form the common madder-red colour.

It is important to inform the dyer, that in consequence of the dry nature of munjeet, it does not easily take up water, especially when it is cold. It must, therefore, be digested with some hot water, or boiled, or macerated with cold water, before it is employed in dyeing.

Opinion of Herrn Dannenberger.—Two trials indicated that the root dyed well by itself, and that the stalks possessed very little colouring matter. One part of cloth impregnated with mordant was dyed with two parts munjeet, and gave a red colour. The addition of clay produced a pink, but dull shade, which is very sensible to common salt, soap, and the light of the sun. Two parts clay and one part munjeet give a better result than 3 clay to 1 of munjeet; an addition of chalk, or alkaline carbonate, prevents the munjeet from giving any of its colouring matter. The addition of ¼ part sumach to 1 part munjeet produces the most saturated, solid, though brownish colour, while 1 part munjeet to 1 cloth dyes much better than as much munjeet alone; and 2 parts munjeet and ½ part sumach to 1 part stuff give the most saturated colour. If the latter colour be compared with that produced with Dutch madder (value, 19½ Prussian dollars per centner), with the addition of sumach, it will be perceived that 2 parts munjeet give out almost as much colouring matter as 1 part Dutch madder. Its value may, therefore, be calculated at 9⅖ dollars per centner. The price of the finest Silesian autumn red (herbströthe), in Feb., 1834, was 13 dollars. The cause

of the difference between these results, and those of Dr.
Runge appears to be, that in the former, river water was
employed, and by the latter distilled water was used.
When distilled water was substituted, the result was much
more favourable, but not such as to warrant us to say, that
the munjeet was much richer in colouring matter, than
Avignon, or Dutch madder. The addition of malt afforded
a still more favourable result.

Opinion of Herrn Böhm.—1. With equal portions of
colouring matter, although munjeet gives a fine red, yet
that dyed with the madder is much more intense.

2. The munjeet also is dyed rather more into the ground
than the madder. And also,

3. Remains distinct after passing through a clay and
chlorine bath.

Opinion of Herrn Nobiling.—The result of his trial was
not in favour of this dye. The shades produced were be-
tween those of madder and lac dye.

ARTICLE VIII.

Case of Anomalous Cutaneous Disease. By
J. CARSON, Jun. M. D.

THE following is a case of anomalous skin disease which
occurred in my practice, and to which I have drawn the
attention of the Medical Society of this town.

Christian, a Dutch sailor of a lymphatic constitution,
and rather emaciated in his appearance, applied at the
North Dispensary, in the expectation of being admitted as
an in-door patient. He was rather brought there by his
poverty than by any particular urgency of his symptoms;
he spoke very little English, which prevented me from col-
lecting any accurate particulars of his history previous to
the present disease. He states that about 12 months ago
he was seized with pains of the joints and limbs of both
upper and lower extremities, which were accompanied by
eruptions of what he termed *Roth Flecke*, of which he
shewed some on his chest. The pains had continued until
he had lost, in a great measure, the power of his hands,
which he uses as if labouring under partial paralysis of the

flexors of the fingers. The spots spread circularly in every direction, at first preserving a perfectly round form, but as they increased becoming more irregular. The small spots now present the appearance of a pale red scale, hardly differing except in increased degree of thickness from the natural epidermis. The colour of the scale, of course, depends on the cutis below it. This appearance surrounds the larger spots for about a line in breadth, and the extension of the disease depends on the eccentric progress of such a line of squammification, if I may so express it. The parts which have been affected present a remarkable contrast, in appearance, to those which are healthy, something like that between the healthy skin and that covering an old cicatrix. The hairs do not seem to grow on it with the same degree of strength, or in the same number; it has none of the oily appearance depending on the various secretions of the skin, and I have never been able to discover any perspiration on it when the neighbouring parts were so affected.

All those parts which have been affected, and which in different places cover a considerable portion of the skin, are so completely insensible, that pinching or burning with heated iron was not perceived by him. This fact, I have repeatedly established in the presence of several medical gentlemen to their entire satisfaction.* The region of insensibility is distinctly limited by the small line of scales. From some discrepancies in his story, I was induced to believe at first, some disposition on his part, to deceive. I several times applied the hot iron to the spots on the back, when his attention was occupied by another in conversation, and when he had not the least reason to expect that such was my intention, without his evidencing, by the the slightest gesture, his consciousness of sensation. I am inclined to think, that the discrepancies above alluded to, arose from his not comprehending my question. His general health is evidently bad, though I was not able to

* I have, by including the cellular substances, and some of the muscles of the back in the fingers, and pinching them hard, attempted to ascertain how deep the insensibility existed. He never complained of any pain, yet, I think, seemed to be conscious we were handling the parts. In an operation, unless some nerves be cut, the patient is, in general, scarcely conscious of pain in the section of these parts.

ascertain, that the functions of any organ were performed with any remarkable degree of irregularity. He complained of no pain in the head, and the region of the spine was not more than ordinarily sensible. He has had the venereal disease severely, some time ago. His hair is of a rusty red colour and frizzled, so as exactly to resemble that of a negro. He said, when I asked him if it had always been so, that his mother was a worker in wool, and this had arisen therefrom. The nails are ill formed and small, but not so much, as to attract particular attention. He has for the last eight years, laboured under a degree of hemeralopia. In the day, the expression of the eye is vivid, and, is, indeed, the best feature in his face which has a coarse, unhealthy scrofulous appearance. A friend of mine, Dr. Wolfe, a German Physician of this town, says, he is confident of having seen a similar case at St. Louis, under the treatment of Alibert. I have some confused recollection of the same kind. From his being removed into an hospital from under my care, I can give little information as to the treatment, but, I believe, it was limited to the use of the warm bath, internal use of arsenic and generous diet. I have learned, that the case is in London, and has been noticed by some of the periodicals.

Article IX.

Comparitive success of Lithotrity and Lithotomy. *

According to Velpeau, it appears that in 1827 Civiale had operated by lithotrity upon 87 patients. Of these 38 died, 3 retained the calculi, 42 were cured, and 19 of them met with severe accidents. In 1830, a new list of 24 calculous cases exhibited 13 cures and 11 deaths; and a subsequent list of 53 cases, had 30 cures, 15 deaths; in 8 the stone was retained. In a 4th statistical table published by Ledain, of 30 cases, 18 were cured, 8 died, and 4 retained the calculi. Of 14 cases treated by Bancal, 2 were cured, all the rest died or retained their calculi. Omitting Bancal's cases; this gives us a total of 194 cases—103 cures and 72 deaths, or the deaths to the cures are as 1 to 1·43.

* Bibliotheque Universelle, April, 1835.

Lisfranc answers this statement of Velpeau from Civiale's paper in the *Dictionaire de Medicine*, from which it appears, that during 8 years from 1824, Civiale has treated 429 patients, comprehending 14 children, 190 adults, and 225 aged persons, or 419 males and 10 females. Of these, 244 were operated on by lithotrity, 236 were cured, 5 died, and 3 continued to suffer. Of the remaining 185, 88 were cut, of whom 48 died, 32 were cured, and 8 were not benefited. These operations were performed, 13 by the lateral section, 9 by the bi-lateral method, and 39 by the hypo-gastric proceeding. In 27 of the latter cases, the issue was not known. The statement of Velpeau with regard to Bancal's cases is not correct: 4 recovered instead of 2. Hence, the mortality of Civiale's cases was trifling, or 1 in 27.

Lithotomy. According to Velpeau, there were, at the Hopital de la Charité from 1719 to 1728, 1200 operations of lithotomy, of which 945 were cured, and 255 died. At Luneville, of 1629 cases, Saucerotte had 1482 cures. Dupuytren lost by this operation 61 out of 356. At Norwich, of 506 operations, there were 70 deaths ; Leeds, 197 operations, 28 deaths. Cheselden had 213 operations and 14 deaths; Frere Come 100 cases, 19 deaths ; Souberbielle 133 cases, 17 deaths ; Dupuytren, by the lateral method, 70 cases, 6 deaths. Cross, in his Jacksonian prize essay, describes 704 operations, and only 93 deaths among these. Renzi, at Naples, had 389 cases, 60 deaths. Pajola lost 5 in 50 ; Panza 5 in 70 ; Ouvrard 3 in 60 ; Virice 3 in 83 ; Martineau, in England, 2 in 84 ; Dudley, in America, 1 in 72 ; Smith, in America, 1 in 18 ; Chelius, in Germany, 1 in 22 ; Petrunti, at Naples, 1 in 25, in private practice ; and Santoro 1 in 56.

To these statements Lisfranc answers, that from 1720 to 1727, there were only 208 operations, at the *Hopital de la Charité*, and 71 deaths, that is, 1 in 3. Moraud gives another table from the Hotel Dieu of 604 operations, in which there were 184 deaths = 1 in 3¼. Another table, at La Charité, from 1731 to 1735, gives 72 operations, and 32 deaths. The success of Saucerotte is accounted for by the number of females operated on, viz. 65, on whom the operation is much less dangerous, and of these 65 only two died. Then there will remain 1564 males cured, and 145 deaths, or 1 in

11. Of thèse 1564, 1119 were below 13 years of age, and only 66 cases between 41 and 78 years. The English cases are also accounted for on the same principles. Lisfranc gives little credit to the statements of Cheselden, Petrunti, Pajola, &c., for, according to the most recent returns from the Neapolitan hospitals, the loss was 1 in 7. He infers, therefore, that Velpeau has under-rated the success of lithotrity, and over-rated that of lithotomy. He observes, that lithotrity is indicated, 1. Where the calculus is small, and the urinary organs healthy. 2. Calculus a little larger, with a healthy bladder. 3. Two small calculi, healthy bladder. 4. Three small calculi, bladder healthy. 5. Calculus the size of a nut, but soft, urinary organs healthy. And lithotomy is indicated, 1. When the calculus is large and hard. 2. Calculus flat (mural). 3. Calculus large, with vesical catarrh. 4. Two large calculi. 5. One calculus filling the bladder.

ARTICLE X.

ANALYSES OF BOOKS.

I.—Philosophical Transactions of the Royal Society of London
for 1835, Part II.

PHYSICS.

On the Ice formed under peculiar circumstances at the bottom of running water. By the Rev. JAMES FARQUHARSON, of Alford, F. R. S.

THE phenomenon of the formation of ice at the bottom of running water, some years ago, attracted the attention of Knight and M'Keever. M. Arago, in the Annuaire, for 1833, collated the opinions of these writers in reference to the subject, and brought forward a number of additional circumstances from observations made in Germany, in order to elucidate its nature. He formed no decided conclusion, but referred to three circumstances, which were partly explanatory of the formation of ice in running water. 1st. The inversion by the motion of the current of the hydrostatic order, by which the water of the surface cooled by the colder air, and which, at all points of the temperature of water under 39° F., would, in still water, continue to float on the surface, is mixed with the warmer water below, and thus, the whole body of water to the bottom is cooled alike, by the mechanical action of the stream. 2d. The aptitude to the formation of crystals of ice on the stones, and asperities of the bottom, in the water wholly cooled to 32°, similar to the

readiness with which crystals form on pointed and rough bodies in a saturated saline solution. 3rd. The existence of a less impediment to the formation of crystals, in the slower motion of the water at the bottom, than in the more rapid one near, or at the surface. Mr. Farquharson, however, considers that these propositions, neither separate, nor combined, are adequate to account for the phenomenon, and conceives that the question should be simply resolved into, " *Why is ice formed sometimes on the surface of running water, and sometimes at the bottom?*" The ice found in this condition is termed, by the Germans, *Grundeis*, and in Aberdeenshire, *Ground-gru; gru* being the term applied to snow saturated with, or swimming in water. The author has seen it formed only when the temperature of the whole mass of water was reduced to, or nearly, to 32°, and when the temperature of the air was several degrees below that point, and observed that it was preceded by a continuance, for some time, of a clear, or very nearly clear, state of the sky. He brings forward a number of observations made by himself, which are decidedly opposed to the explanations of Arago. At sunrise, on the river Leochal and Don, when the temperature of the water was 32°, and of the air 23°, much *ground-gru* was observed at the bottom of the water; at 10 A. M., a cloud obscured the sky, and at 2 P. M. the air was at 40°; much *gru* rose to the surface, and floated down the stream. Before sunset, the thermometer was 31°. On two subsequent days, the temperature was lower, with a clear sky. The bottom of the river was now much impeded by the *gru;* and, what is worthy of notice, the clear spaces of the bottom, at the piers, abutments, &c. of the bridge, on the Don, still continued clear, but were much encroached upon, on the sides next the streams, by the *gru.* Next day a thaw occurred, when the thermometer rose to 47°; the rivers were cleared of ice and *ground-gru*, which floated away. On the 2d day, the temperature was 29°, snow fell, and was entangled in many parts of the rapids; but there was no appearance of the symmetrical cauliflower shaped *ground-gru.* On the same evening, and two following days, the temperature fell to 23° and 21°; the sky was clouded; the rivers frozen over in many places, but no *ground-gru* could be observed. The snow, however, which had been entangled in the rapids disappeared to a great extent, obviously floating away in the stream. He states also, that plants in the bed of the stream were covered with *gru*, while none was observed in the bed itself. The shaded parts of the stream were also free from *gru*, while it was abundant in the free portions. The answer to his original question, the author considers is, that ice is formed sometimes on the surface of running water, and sometimes at the bottom, because frost sometimes takes place with a clouded sky, which is incompatible with radiation of heat from the bottom of the stream, and sometimes with a clear sky, when that radiation takes place through the water, in the same way as through air. The bottom is thus cooled below the freezing point of water, before the water itself; ice is formed on it, and its detachment, by transmitted heat from below, prevented, as long as the radiation continues.

But, there is still a point of importance to settle. Why does de-

position not occur in still, rather than in running water, since radiation would be more plainly manifested in the former, than in the latter? The author answers, that "in still water, the hydrostatic order which M. Arago has so well illustrated, as belonging to water, when reduced to a temperature under 39°, has free play to establish itself, and is not inverted by the mechanical action of a stream. When the temperature of a body is under 39°, then the coldest portions of it are the lightest, and naturally rise and float on the surface. When, in a still pond, the water nearest the bottom has been cooled below the general temperature, by contact with the solid materials cooled by radiation, it is displaced by the heavier, warmer water above. Hence, ice forms first on the surface, by the meeting there of both the cold of radiation, and that acquired by contact with the incumbent cold atmosphere." "In the rapids, the hydrostatic order is overturned, and the colder, which is also the lighter water, not only mixed with the warmer below, but at the whirls of the greatest rapids, brought suddenly, without much mixing, into direct contact with the bottom cooled still lower than itself by radiation. If the water is at the temperature of 32° F., it can give out no heat to the colder bottom without part of it being converted into ice, the spiculae and crystals of which find a solid body for their attachment, at the very point where the heat is given out."

Discussion of Tide Observations made at Liverpool.
By John William Lubbock, Esq.

This paper consists of a number of tables, drawn up from observations made at the London and St. Katharine's Docks. Those at the former, are made by a person who notes the time when the water has begun to fall, that is, *has made its mark.* The observations at the latter, are made by noting upon a slate, (ruled for the purpose) the height of the water, every minute before high water is expected, all which, is afterwards copied into a book ruled in the same manner, and the time of high water with the height, is easily inferred. The height is ascertained, by means of a rod or tide-guage, connected with a float, which is placed in a chamber, into which the water enters through a culvert, so that the ripple or agitation of the water in the river is avoided, as much as possible; a clock carefully regulated, stands close at hand.

Experimental Researches in Electricity, 10th Series.
By Michael Faraday, D. C. L., &c.

The object of this paper is to describe an improved plan of the voltaic battery, and to develope some practical results, in reference to its construction and use. In a simple voltaic circuit, the chemical forces, which during their activity give power to the instrument, are generally divided into two portions; the one of these is exerted locally, while the other is transferred round the circle; the latter, constitutes the electric current of the instrument, while the former, is completely lost or wasted. The ratio of these powers varies; thus, in a battery

not closed, the *entire* action is local—in one of the ordinary construction, *much* is in circulation—and, in the perfect one, *all* the chemical power circulates and becomes electricity. By estimating the quantity of zinc dissolved, and the quantity of decomposition effected in the volta-electrometer, these proportions can be appreciated. If a voltaic battery were formed of zinc and platinum, the latter metal, surrounding the former, as in the double copper arrangement, and the whole being excited by dilute sulphuric acid, then no insulating divisions of glass, porcelain, or air, would be required between the contiguous platinum plates, and provided these did not touch metallically the same acid, which being between the zinc and platinum, would excite the battery into powerful action, would between the two surfaces of platinum, produce no discharge of the electricity, nor cause any diminution of the power of the trough. This is a necessary consequence of the resistance to the passage of the current, which occurs at the place of decomposition, for that resistance is fully able to stop the current, and, therefore, act as insulation to the electricity of the contiguous plates, as the current which tends to pass between them, never has a higher intensity than that due to the action of a single pair. If the metal surrounding the zinc be copper, and if the acid be nitro-sulphuric acid, then a slight discharge between the two contiguous coppers does take place, provided there be no other channel open, by which the forces may circulate, but when such a channel is permitted, the return discharge is much diminished. Upon these principles, the author was led to construct a trough, in which, the coppers passing round both surfaces of the zincs, were only separated from each other by paper. He soon found, that this was exactly Hare's trough. It is very convenient, for when composed of 40 pairs of 3 inch plates, it can be unpacked in five minutes, and re-packed in half an hour. Its effect on platinum wire, in the shock, &c., was equal to 40 pairs of 4 inch plates with double coppers, in porcelain troughs. With 20 pairs of 4 inch plates arranged in Hare's trough, and 20 pairs of 4 inch plates in porcelain troughs, there was a consumption by the former, of 3·7 atoms zinc, and 5·5 for the latter. Hence, no doubt can exist, of the great superiority of Hare's method of arrangement. This plan will, therefore, soon supersede the old method, as 100 pairs of plates need not occupy a trough of more than 3 feet in length ; and, by making it turn upon a pivot, the acid may be poured off when required. The author recommends troughs of porcelain, because it is difficult to make a wooden one constantly water tight.

Under distinct heads, the author describes some important practical points.

Nature and strength of the acid.—Of all the acids singly, nitric acid answers best. It improves the action of sulphuric acid. The proportions employed by Dr. Faraday for ordinary purposes were 200 water, 4½ sulphuric acid, and 4 nitric acid. The quantity which each zinc plate lost in these circumstances was 2·16 atoms; with double the quantity of acids and the same proportion of water, the loss was 2·26 atoms. No copper is dissolved during the regular action of the trough, but much ammonia is disengaged when nitric acid is present.

Character of the zinc plates.—If pure zinc could be obtained it would be highly advantageous, as the foreign metals, such as copper, lead, iron, cadmium, &c., which are left on the surface of the acid, to whose action it is exposed, diminish the effect of the action. The purest zinc of commerce is the rolled Liege or Mosselman's zinc. After being used, the plates of a battery should be cleaned from the metallic powder on their surfaces. No old charge containing copper should be used to excite a battery. New plates are much more powerful than such as have been used. The first time, 20 pairs of 4 inch plates in porcelain troughs were used, they lost per plate, only 3·7 atoms; but after that the loss was 5·25 to 5·9 atoms.

Vicinity of the copper and zinc.—When the copper and zinc are near to each other, the power is not only greater at the instant, but also, the sum of transferable power, in relation to the whole sum of chemical action at the plates, is much increased; because, whatever tends to retard the circulation of the transferable force, (viz. electricity), diminishes the proportion of such force, and increases the proportion of that which is local. Now, the liquid in the cells produces this retarding power, and, therefore, acts injuriously, in greater or less proportion, according to the quantity of it between the zinc and copper plates, *i. e.* according to the distances between their surfaces. The superiority of *double coppers,* also, depends, in part, upon diminishing the resistance offered by the electrolyte between the metals. The great effect, on first immersion, is owing to the unchanged condition of the acid, the effect of which diminishes as it becomes neutralized.

Number and size of plates.—The author found that the consumption of zinc, arranged as 20 plates, was more advantageous than if arranged either as 10 or 40; and also, that increase of numbers did not improve the effective production of *transferable chemical power,* from the *whole quantity of chemical force* active at the surfaces of excitation. If, in a particular case, the most effectual number of plates is known, then the addition of more zinc would be most advantageously made in increasing the *size* of the plates, and not their numbers. At the same time a large increase in the size of the plates would raise, in a small degree, the most favourable number. Large and small plates should not be used together.

Simultaneous decompositions.—When the number of plates much surpasses the most favourable proportion, two or more decompositions may be effected, and simultaneously, with advantage. Thus 40 pairs of plates produced 22·8 cubic inches of gas in one volta electrometer; when re-charged, they produced 21 inches in each of 2 electrometers. When 20 pairs of 4 inch plates were used, the results were different; with one electrometer 52 cubic inches of gas were procured, with 2 only 14·6 cubic inches. These results depend upon the same circumstances of retardation as have been already mentioned.

(To be continued.)

II.—*The American Journal of Science and Arts, for July, August, and September,* 1835.

In March last, Dr. Silliman, the editor of this journal, since its commencement, 17 years ago, made an open appeal to his countrymen in

favour of his work; he told them candidly, that unless it was re-invigorated by additional support, it could not be permanent; and that, should it be left to die of penury in the midst of abundance, he would exonerate himself from blame, and lay both the injury and the dishonour at the door of his country. He pointed out a simple plan to his subscribers, by which each individually could assist his cause, and the honour of his country, which was at stake. His suggestion was, that *each subscriber should kindly endeavour to obtain one more.* This was enough. Our Transatlantic friends did exert their energies *immediately*, as is their constant practice in the cause of improvement. They said, " A Scientific Journal shall not be allowed to die, because we are sensible, that such an organ is a necessary auxiliary to science, and because we are able to support it." In July, the permanence of the work was almost secure. What has been the consequence of this activity? The subsequent Number, whose title we have placed at the head of this article, supplies us with an ex-cellent geological description of the coal deposits of the Ohio, illus-trated with 36 plates of fossils, and a map. A treatise of the same nature would, in this country, have been locked up in the expensive volumes of one of our societies, for the perusal of a very limited number of persons.

It may be true, that America has contributed but little to the ad-vancement of science; but it is a most striking fact, that science is there much better supported, and is more promising than in this country.

But we leave this subject, and proceed to the consideration of the coal bason of the Ohio. The district, described by Dr. Hildreth, extends over a space of 4 or 5 degrees in latitude, by as many in longitude, and includes the north-west portions of Pennsylvania and Virginia, with the north-east of Ohio, and a small tract in the north-east of Kentucky; being traversed by the Ohio, from Pittsburg to Burlington and Portsmouth. The appearance of the country is that of an undulating plain; long sloping ridges running parallel into the river, and increasing in height in proportion to their distance from it. The surface rocks are sand-stone commonly, but no primitive rocks have been observed, even, although the depth of 1000 feet has been attained. The strata are, in general, little disturbed, and appear to have been formed slowly. The principal river from the west is the Muskingum, whose limpid water is charged with carbonate of lime. The aspect of the country, through which it passes, is hilly and broken, but on the head branches, the surface is more level. The plains are covered with fragments of gneiss, mica-slate, granite, and green-stone, mixed with alcyona, madreporites, corrallines, and shells, the tenants of an ocean, which at one time covered this country. The trees, which adorn the immense forests, are now rapidly disap-pearing before the hand of cultivation, and consist of *Liriodendron tulipifera*, or yellow poplar, *Magnolia Acuminata*, or cucumber tree, *Cornus florida, Cereis Ohioensis*, or Judas tree, the American date tree, hickory, sugar trees, spicewood, beech, yellow pine, *Kalmia latifolia, &c.*

The north-west portions of the valley of the Muskingum belong to supercretaceous formations, the south and east to the carboniferous

series, and the extreme southern border to the new red-sand-stone group. At the mouth of the river, an interesting grotto of plants has been discovered. The sand-stone rock, in which it occurs, is 50 feet thick, and rests on slaty marl. Many of the plants appear to be aquatic vegetables; but the most abundant are of the genus Neuroptera (of which excellent wood-cuts are appended). Thin beds of coal appear near this at the surface. Ascending the Muskingum extensive deposits of salt occur, at the depth of 620 feet; 50 gallons of water containing 50 pounds of fine salt. On all the eastern branches of this stream, coal is abundant, but becomes more scarce as we approach Lake Erie. On the borders of the coal region, iron ore is abundant, and is extensively worked. Marine fossils occur both above and below the coal, and through all the coal region many proofs exist of the action, both of fresh and salt water. Sections of the hills, in the neighbourhood of the river, present alternations of clay, sand-stone, coal, and limestone, containing *encrinites, grypheæ, &c.*

Cannel coal is met with at Cambridge, in Guernsey county; this constitutes the only known locality for it in America. The valley of the Hockshocking river is similar to that of the Muskingum river.

On the east side of the Ohio, one of its most powerful tributaries is the Inonongahela, whose valley occupies a space of about 180 miles in length, and lies between the Alleghany mountains, and the Ohio. The waters of this river pursue a course directly the reverse of those of the Ohio. The formations through the whole extent of this valley are recent secondary, consisting generally of sand-stone. The prevailing colour is light gray. The dark brown or red variety, known as the old red sand-stone, is seldom seen except in some of the mountain ranges, and is strictly a transition rock. The sand-stone often alternates with coal, shale, limestone, and marl. Fifteen miles from Wheeling, the main surface coal deposit dips under the bed of the river, and is not again seen in any considerable quantity until it appears at Carr's Run, 150 miles below. The same deposit extends into Ohio, and is found in great abundance about St. Clairsville. Hence, it is at least 200 miles long, and 100 broad; and affords one of the most extensive coal fields known in any part of the world. At Morgantown, there are no less than 3 beds of coal above the surface of the river. The 1st bed is at the elevation of 300 feet, and is 6 feet thick, of moderately good quality; the 2d at 150 feet above the river, 7 feet thick, of excellent quality; the 3d at 30 feet height, 3 feet thick; No. 4. a few feet below the surfcce, 7 feet thick, and excellent; No. 5. at 147 feet, said to be 30 feet thick, but of inferior quality. At Pittsburg, also, five exposed beds occur. The banks of the Kiskiminitas afford sections of thick beds of coal, shale containing fishes, and limestone. The coal beds vary from 1 to 12 feet in thickness, but rarely exceed 6. The space occupied, by the coal in this vicinity, does not fall short of 21,000 square miles. In consequence of its occurring in abundance above the surface, it is mined to a very great extent, at the rate of 1 and 2 cents per bushel; and is thus brought within the means of *all*, and

literally to every man's door. Its cheapness has given rise to a variety of manufactories. Pittsburg and its environs contain 90 steam engines, which employ, with other sources of consumption, annually 255,500 tons of coal. This, at 4 cents per bushel, the price in Pittsburg, is equal to 306,512 dollars (about £68,000). The coal consumed in the western countries in the manufacture of salt is very great. There are above 90 establishments which produce 1,000,000 bushels of salt, and consume 5,000,000 bushels of coal. What a splendid prospect of future prosperity do these statements hold out!

At Wheeling the coal exposed to view is 7 feet thick. The scenery of the Kenawha valley is varied and beautiful. The surface is covered with the *Chionanthus,* or Fringe tree, *Magnolia tripe-tala, acuminata and mycrophylla, Rhododendron maximum, Kalmia latifolia, &c.* The rocks are shale, coal, and sand-stone. The former, affords an abundant supply of fossil plants, including, *Equisetum columnare? Calamites Steinhaueri, C ramosus, C arenaceus? Sphenopteris crenulata, Neuropteris acutifolia, Sphenopteris obtusiloba? &c.* Salt water is found abundantly in this valley. It is found at considerable depths, associated with red marl in alternations with sand-stone, slate clay, limestone and coal. Gypsum has not been found to accompany the rock strata near the surface of the earth, in the valley of the Ohio, although indications of it are found at great depths, and extensive beds of it are deposited on the borders of the valley, in the secondary and transition rocks at the surface, and, hence, may be deposited beneath the series of sand-stones and coal throughout the whole valley. It occurs in abundance along the south shore of Lake Erie, to the interior of New York State. No shafts have been hitherto sunk to determine whether the salt exists in strata or not, but there can be little doubt of the fact, from the abundant supply which is met with.

The Indians, it would appear, from the earliest times, were in the habit of sinking in the gravel near the river, "Gums," or hollow logs for extracting salt. In 1795, Joseph Ruffner located on the Kenawha; but, it was not till 1807, that his sons, David and Joseph, selected a gum or hollow sycamore, 18 feet long, and 3 feet diameter; which they sunk, with great labour, to the depth of 14 feet in the sand, a little above Buffaloe Lick, down to the smooth sand-stone forming the bed of the river. This rock was bored by means of an auger or chisel passed through a tube. At 17 feet, they struck a vein of salt water, the first indication of which, was a bubbling or hissing of gas in the hole. It was sunk to 26 feet. A furnace was then erected of about 40 kettles, which began to work, in February, 1808, and made about 25 bushels of salt per day, value 2 dollars per bushel, at that time. The wells are now made deeper, coal is employed, and in 1834, a million and a half of bushels were raised. The Kenawha presents a lively and interesting scene of activity and industry, with its steam boats, and its banks lined with furnaces and railroads.

In Europe the salt is said to occur *above* the coal only, but we have doubts of the accuracy of this statement. The facts detailed

by Dr. Hildreth demonstrate, that it may be found also *below* coal ; and, that what our geologists would term new red sand-stone is in reality associated with [coal. When the sand-stone assumes a red colour, it would appear to prove that the agency of heat has been exerted in its neighbourhood, for, when we ignite various varieties of white sand-stone, they assume a red aspect. The saline fountains in Ohio are characterized by their evolving quantities of carburetted hydrogen, which brings up with it large quantities of petroleum.

We must now conclude this notice, but cannot do so without expressing the high degree of satisfaction we have received from the perusal of the geological memoir of Dr. Hildreth. If his knowledge of fossils had been equal to his powers of observation, the paper would have been still more valuable. We can learn from his observations, however, that he does not confine his researches to the mere external description of masses of stones; but that he brings in to his assistance, the collateral sciences, without whose aid, geological details, as they are given too often in our own country, are mere lumber. The illustrating lithographs and wood cuts appended to the memoir are excellent specimens of the American arts.

III.—*The Doctrine of proportion, or Geometrical Admeasurement by similar triangles, practically applied to expanding or diminishing Drawings.* London, 1836. Ackermann and Co.

THE object of this publication is to fix the principles of drawing upon mathematical, and, therefore, upon fixed and invariable principles. It is just such a work as we have long wished to see; and we have no doubt, that it will be properly appreciated, by those for whom it is intended, viz., all who wish to draw correctly and scientifically.

ARTICLE XI.

SCIENTIFIC INTELLIGENCE.

I.—*Pharmaceutical Preparations.*

1. *Indigo in Epilepsy.*—Dr. Ideler, of Berlin, has treated 26 cases of epilepsy with indigo; of these, 6 were cured without a relapse, 3 had relapses after some months, 11 were much relieved, and upon 6 no effect was produced. His formula is, R *Pulv. Indig.* oz. $\frac{1}{2}$, *Pulv. Aromat.* dr. $\frac{1}{2}$, *Syrup. Simp.* q. s. ut ft. electuar. To be taken at first in two days, and then in a single one. The dose of indigo may be increased to 6 or 8 drachms per day. The first effects are nausea and vomiting. It also induces constipation ; the urine is brown.—*Rust's Magazine.*

2. *Antiseptic Liquid.*—M. Lereboullet, Conservator of the Museum of Natural History at Strasbourg states, that for two years the anatomical preparations in this establishment have been preserved by a liquid consisting of chloride of calcium 4 parts, potash-sulphate

of alumina 2 parts, nitrate of potash 1 part, water 16 parts. M. Vinet, keeper of the museum, has also used it for tanning skins which are to be piled up. It is particularly useful in preserving the brain. —(*Journ. de Chim. Medic.* i.)

3. *Caustic powder of Vienna.*—This consists of caustic potash with lime 5 parts, pulverized quick-lime 6 parts. When it is to be employed, it is mixed with a little alcohol so as to form a liquid paste, which is spread between two pieces of cerecloth, one of which has an aperture in it of the size and form which we wish to give to the eschar. The action is rapid but not painful, and always terminates in less than half an hour.—(*Ibid.*)

4. *Hydro-ferro-cyanate of Quinin.*—Ferrari forms this preparation by employing equal parts of pure quinin and Prussian blue in fine powder. The quinin is dissolved in alcohol, hydro-cyanate of iron is then added, and the whole is boiled for some minutes. The boiling liquid is then filtered. It has a greenish yellow tinge, is soluble in water, and presents all the characters of hydro-ferro-cyanate of quinin. In this process, the Prussian blue employed has lost the fourth of its weight, so that if 4 parts of quinin are employed, the new product weighs five.—(*Ibid.* i. 360.)

5. *White lead Plaster.*—Ferrari recommends that this plaster should be made by boiling together 6 lbs. of olive oil and 3 lbs. of lead. During the boiling, acetic acid is to be added. This will be decomposed and form carbonic acid, which will combine with the oxide of lead. Instead of acetic acid, sugar of lead, with a smaller proportion of lead may be employed.—(*Gazetta eclettica di Farmac.* iii. 166.)

II.—*Progress of Science.*

THE year 1833 furnished important descriptions of the 3 peninsulas of Southern Europe, Mexico, South America, and Hindostan. England afforded 45 publications of this kind, France 46, Germany 31, Italy 19, Russia 15, and the United States 16. During the same year, there appeared in the Physical and Natural Sciences, 144 treatises, and 276 memoirs in the whole; and in Geology and Palæontology 61 treatises and 414 memoirs; or, in the whole, 205 treatises and 690 memoirs; or, 895 publications. In comparing the number of works of 1833 with those of 1830, 1831 and 1832, the ratio is expressed by the following figures: 300, 450, 500 and 900. —(*Silliman's American Journal,* xxviii. 294.)

III.—*Carices common to North America and Great Britain.*

PROFESSOR Dewey received a collection of 190 Carices which had been collected in the voyages and tours in Arctic America, from Dr. Hooker through Dr. Torrey. Those common to America and this country are: *C Dioica*, Rocky mountains; *Davalliana* ib; *Capitata* ib; *Incurva* ib; *Curta* ib; *Saxatilis* Bear Lake; *Acuta* Columbia river; *Cæspitosa* rocky mountains; *Atrata* ib; *Vahlii*

ib; *Œderi* Canada; *Pallescens* Carlton-house; *Capillaris* Rocky
mountains; *Limosa* Hudson's Bay; *Pseudo-cyperus* Cumberland-
house; *Filiformis* Cumberland-house; *Ampullacea* Bear Lake.—
(*Silliman's American Journal*, xxviii. 270.)

IV.—*Royal Institution.—22nd January.*

On Silicified Fossils.—Dr. Faraday began his observations on
the conversion of ancient woods into siliceous matter, by detailing the
characters of silica, its insolubility in acids when in a dry pulverulent
state, and its ready solubility in water when fused with an alkali,
alluding, in passing, to the method which we possess by means of
fluor spar of obtaining it in an elastic, gaseous form. Whether it can
be sublimed by the direct agency of heat seems doubtful. Dr.
McCulloch relates an experiment in which he exposed silica to a
strong heat in a crucible, and apparently sublimed a portion. It is
possible, however, that in this instance there might have been some
fallacy. Dr. Faraday exhibited specimens of flint, from the chalk,
agates which may be considered a kind of siliceous nodules occurring
in trap rocks, and a beautiful example of cap rock crystal to illustrate
the deposition of the siliceous matter in layers. He shewed also a
fine amethyst containing colourless layers on its surface, which were
thicker on some places than on others, and gave it as his conclusion
drawn from an inspection of all the different forms of silica, that it
had been deposited by one law, because we find agates, chalcedony,
rock crystal, and other forms, united in the same mass. The for-
mation of chalcedony, he considers, cannot be accounted for by the
mere drying of gelatinous silica, the contraction which would follow
being too great to correspond with the forms in which we find chal-
cedony.

Silicified woods are found lying on the surface of siliceous and cal-
careous formations, as in Africa and Antigua. In some specimens
we find that the soft parts of the wood yield first, and are replaced
by silica, while in others, we observe the hard parts giving way and
the soft parts remaining. In others again, both hard and soft por-
tions have disappeared, and have been entirely replaced by silica. Spe-
cimens from Antigua exhibit trees silicified in all stages of decay. In
one specimen which the lecturer shewed, the exterior circles were
silicified, and exhibited the vessels of the plant in perfect preserva-
tion, while the centre had been hollow and was filled up with agate.
There is no evidence to prove that silicification has taken place in mo-
dern times. The effect produced by the Geyzers is merely incrusta-
tion, for the substance upon which the silica from these springs is
deposited remain entire; silicification, however, consists in the dis-
placement of organic matter by silica. Several instances have been
related of the effect of rivers in silicifying or petrifying with silica, as
of the waters of the Aar, Danube, and Loch Neagh; but the loca-
lities to which this power was assigned, when examined by competent
authorities exhibited no such property. Specimens of what have
been considered by some as silicified sugar canes were shewn by Dr.

Faraday, but the vessels in these specimens, as he was informed, were quite different from those of the recent sugar cane.

The examples which appear to be of the most recent formation, are some specimens from Loch Neagh, where fibrous portions of carbonaceous matter occur dispersed in different parts of silicified masses, and the silicified *Gorogonites*, or seeds of the *Chara hispida*, described by Mr. Lyell, from the lakes in Forfarshire.

In referring to the explanation of these curious phenomena, Dr. Faraday considered that the present state of our knowledge did not enable us to afford a solution of the difficulty, and that to form a theory would merely tend to embarass the subject, for such was the universal character of theories. It is impossible to admit that intense heat could have produced these changes, because Dr. McCulloch asserts that *confervæ* exist in many specimens of rock crystal, which would have been destroyed if the matter in which they lie enclosed had been exposed to a high temperature. The views of Dr. Turner, the lecturer considered, afforded an excellent explanation of the source of the silica. He exposed portions of crown and window glass to the action of steam in a high pressure boiler, the temperature being 300° F. In the course of 4 months, they were found to be more or less decomposed ; the white earthy portions were found to be entirely free from alkaline matter, but the actual loss was not due to the extraction of the alkaline matter only, for the silica of the glass had also been dissolved, as was proved by the apertures of the wire gauze in which the glass was incased, being filled up at the most depending parts by a siliceous incrustation, where also a stalactitic deposit of silica about an inch and a quarter long had formed. Dr. Turner adduced these facts to illustrate the action of water at high pressure on felspar, and other rocks containing alkaline matters. Dr. Faraday considered these experiments as highly important in regard to affording a source for the silica, but conceived that we were still ignorant of the mode in which the silica is deposited in such a variety of forms. He stated that he had brought the subject before the meeting to stimulate to investigation respecting this interesting and beautiful phenomenon.

Note.—It may be observed, that excellent imitations of chalcedony can be produced, by allowing silica, in the gelatinous state, to dry on a filter in the open air. The following queries may not, perhaps, be out of place : Is the opinion expressed by Dr. Faraday on the influence of theories altogether just ? Have they not acted beneficially on the developement of electricity, optics, and chemistry ? Does the explanation suggested by Dr. Turner apply to the solution of silica in any other situations than under great pressure, and consequently at a great depth, and at a temperature above the boiling point of water ? If it does not, why should silicified wood occur only at the surface, and chiefly on siliceous and calcareous formations ?—Edit.

V.—*Action of Acids upon Sugar.*

Malaguti finds 1st. that both organic and inorganic acids act in the same way upon sugar, when influenced by heat ; and that it is trans-

formed first into sugar of grapes, then into ulmic acid, and (if atmospheric air is present) into formic acid. 2nd. When cane sugar is changed into sugar of grapes, the action of the acids takes place even at common temperatures. 3rd. That the smallest quantity of acid acts in the same manner but more slowly. An acid less dilute will act more quickly than an acid more dilute. 4th. Dilute acids under the action of atmospheric air cannot transform sugar into formic acid. 5th. The action of alkalies upon sugar is identical with that of acids. His experiments confirm the accuracy of the composition assigned to ulmic acid by Boullay, viz. C^2 H O. Ulmic acid may be readily formed by boiling together 10 parts sugar, 30 water, and 1 concentrated sulphuric acid. In three quarters of an hour a scum forms on the surface which may be skimmed off; in a few minutes it is formed anew. This scum is ulmic acid with a little ulmin, which may be separated by ammonia. Water should be added occasionally in order to re-place what evaporates.—(*Ann. de Chim.* lix. 407.)

VI.—*Chloro-benzine and Chloro-benzide.*

Chloro-benzine is formed, by exposing benzine and chlorine in a close vessel to the action of the sun. A white vapour forms and is gradually deposited in crystals. It is insoluble in water, little soluble in alcohol, but more soluble in ether. Fuses at $269\frac{1}{2}°$. It consists of carbon 25·14; hydrogen 2·06; chlorine 72·80. This is equivalent to C^3 $H1\frac{1}{2}$ Cl $1\frac{1}{2}$.

Chloro-benzide may be readily formed by subjecting chloro-benzine to distillation, mixed with an excess of hydrate of barytes or lime. Water, and a chloride is formed, and chloro-benzide passes over. Chloro-benzide is an oily colourless liquid. Specific gravity 1·457. Insoluble in water, very soluble in alcohol, ether and benzine. Not altered by acids and alkalies. Boiling point 410°. It consists of carbon 39·91; hydrogen 1·62; chlorine 58·47. The density of its vapour was found to be 6·37. This corresponds with 6 vol. carbon vapor = 2·4996, $1\frac{1}{2}$ hydrogen = ·991, $1\frac{1}{2}$ chlorine = 3·75 = 6·24.—(*Poggendorff's Ann.* xxxv. 370.)

VII.—*Silica in Plants.*

STRUVE has obtained the following results from an examination of the ashes of several plants.

		Silica	Alumina	Lime	Manganese
Equisetum	hyemale	97·52	1·7	0·69	
,,	limosum	94·85	0·99	1·57	1·69
,,	arvense	95·48	2·556	1·64	
Spongia	lacustris	94·66	1·77	2·99	
Calamus	Rotang	99·20	,,	0·54	

(*Erdmann und Schweigger Seidel's Journal,* v. 462.)

VIII.—*Antimonial Copper Glance, a new Mineral.*

THIS mineral was found by Zinken in drusy quartz cavities, at Wolfsberg. It occurs in the form of four-sided prisms, of which the lateral edges are so much truncated as to give the crystal a tabular appearance. Colour, lead gray to iron black.. Hardness, between calcareous spar and fluor spar, or 3·5. Specific gravity, 4·748. Lustre, metallic, splendent. Fracture, in the long axis of the transverse fracture, foliated faces of cleavage glassy; in all other directions the fracture is more or less uneven.

Before the blow-pipe it decrepitates, and easily fuses in the external flame. On charcoal it gives out the odour of white antimony. Fused with soda it gives a reddish metallic grain, which, by continued heat, gives out an odour. Henry Rose found it to contain from 3·57 to 5·79 per cent. of silica mechanically mixed. Its constitutents are : Sulphur 26·34. Antimony 46·81. Iron 1·39. Copper 24·46. Lead 0·56=99·56. The iron is probably combined with copper and sulphur so as to form copper pyrites, and the lead is probably combined as sulphuret of lead with sulphuret of antimony, forming Federerz. 1·39 parts iron require 1·65 sulphur, and 1·62 copper to form pyrites, while to form Federerz we have 0·56 lead + 0·08 sulphur, with 0·4 sulphuret of antimony. There remain, therefore, 47·46 antimony, 22·84 copper; the first requiring 17·36 sulphur, the latter 5·81 parts. The formula, therefore, is $SbS + CuS$.—(*Poggendorff's Ann.* xxxv. 357.)

IX.—*Metamorphosis of Plants.*

A CORRESPONDENT, *Amicus Physiologicus,* writing from the Isle of Wight, (11th December), objects to the observations of Professor Rennie who considers the ideas of Linnæus, Göthe, De Candolle, Lindley, &c., in reference to the metamorphosis of plants, " wildly absurd, as must at a glance appear to every reader endowed with common sense." Our Correspondent relates an experiment which he (?) conceives to afford an argument in favour of the theory in question.

Some rose leaves having been boiled with a portion of alcohol and water, so as to abstract their colouring matter, his attention was attracted to a leaf which had adhered to the side of the vessel in which the experiment was made, and " in that flower leaf was distinctly traced a leaf of the tree which seemed to form the basis of the leaf of the rose." He concludes " that we may safely infer that flowers are transformed leaves."

NEW BOOKS.

Laboratorium, das, eine Sammlung Von Abbildungen und Beschreibungen der neuesten und besten Apparate, 4 tafeln. *Breslau.*

Carus, Dr. C. G. und Dr. A. W. Otto Erläuterungstafeln zur Vergleichenden Anatomie. Barth, *Leipzig.*

HORARY OBSERVATIONS OF THE BAROMETER, THERMOMETER, &c.

(Made at the Manse of the Parish of Abbey St. Bathan's, Berwickshire, Lat. 55° 52′ N. Long. 2° 23′ W. at the height of about 450 feet above the sea, for the commencement of each hour per clock, beginning at 6 o'clock in the morning of Monday the 21st December, and ending at 6 o'clock in the evening of Tuesday the 22d, thus extending over 36 hours, according to the suggestion of Sir John Herschel.)   By the Rev. JOHN WALLACE.

Monday, 21st Dec. 1835.	Hour.	Thermometer.o	Hygr.(Leslie's).o	Barometer at 32o of Fahrenheit.	Direction of the Wind.	Remarks.
A.M.	6	33	2	29·843	W.N.W.	Calm sky overcast; a sprinkling of snow on the ground, recently fallen.
	7	34	5	29·831	W.N.W.	Gentle breeze; clouds breaking in the S. E. qr. a little above the horizon: W. qr. lowering.
	8	34½	4	29·855	—	So calm below that the direction of the wind cannot be noted; but the clouds in motion from N. E. The S. E. clear, the rest of the sky veiled with cirro-str. passing into nimbus. A slight shower.
	9	34¼	2	29·861	W.N.W.	Very gentle breeze; clouds still moving from N. E. Nimbus overspreading the sky with frequent showers.
	10	35	7	29·865	N. W.	Very gentle breeze; sky nearly veiled by an upper stratum of cirro-strat.; large black masses of cloud resembling cirrostratus passing from north-eastward.
	11	35¾	8	29·871	N. W.	Gentle breeze; the general character of the clouds the same as during last hour. The clouds tending to break up, particularly in the zenith.
Noon.	12	36	8	29·872	NWbyW	Gentle breeze; sky completely overcast, but the general aspect of the clouds continues the same.
P.M.	1	35½	12	29·866	N. W.	Gentle breeze; the sky overspread with nimbus, drizzling rain; the clouds continue to move from north-eastward.
	2	35½	5	29·866	N. W.	Breeze somewhat increased; drizzling rain. Nimbus dissipating in zenith.
	3	34¾	5	29·869	N. W.	Gentle breeze; tending to clear in S. W. and zenith. Nimbus passing into cirrostratus.
	4	35¼	8	29·869	N. W.	Gentle breeze; an extensive bed of cirro-strat. formed in S.W. qr.; the rest of the heavens overspread with nimbus; slight drizzling rain. The clouds by their motion still indicating a current from NE.
	5	34½	4	29·890	N. W.	Until P.M. (inclusively) the general aspect of the heavens and character of the clouds remained the same as described for the preceding hour.
	6	34½	4	29·890	N. W.	The same as four o'clock.
	7	35¾	7	29·900	NWbyW	The same.
	8	35¾	7	29·903	NWbyW	The same.
	9	36	7	29·914	W. by N.	Gentle breeze; with slight tendency to clear overhead.
	10	36¾	7	29·925	W. by N.	Calm; sky again overcast, slight drizzle.
	11	36¾	5	29 925	W. by N.	Very gentle breeze, rain during a great part of the past hour; now fair, with some tendency to clear overhead.
	12	35¾	5	29·935	W.	Gentle breeze, sky overcast drizzling rain; a lightness on the E. qr. of the horizon under a dense black cloud.
Tuesday. A.M.	1	36	7	29·941	W.	Gentle breeze; clearing from the zenith towards the western qr. of the horizon. The opposite quarter of the heavens still obscured with heavy clouds.
	2	35¾	7	29·950	W.N.W.	Calm, sky clear from the zenith westward to the horizon, but still hazy towards the N. E.
	3	34¼	6	29·950	—	So calm that direction of wind cannot be noted; sky clear, except a haziness in S. E.
	4	33	3	29·951	—	So calm that the direction of the wind cannot be noted; a bed of cirrostratus forming on the horizon from northern towards eastern quarter, detached patches rising towards the zenith.
	5	32½	3	29·946	SW by W	Very gentle breeze, sky cloudless except on the S.E. qr. of horizon.
	6	32	3	29·959	SW by W	The same as last hour.
	7	30½	3	29·969	SW by W	Breeze slightly increased; otherwise the same as last two hours.
	8	29	4	29·987	SW by W	Breeze still slightly increasing; hoar frost on the ground, otherwise the same.
	9	28½	4	29·996	SW by W	The mass of dense thick cloud in S. E. dissipating. Otherwise the same.
	10	30½	5	29·997	SW by W	Gentle breeze and cloudless sky.
	11	31½	6	30·000	SW by W	The same as preceding hour.
Noon.	12	35½	11	29·997	—	Too calm for noting the wind with certainty, but it appears to be shifting south-eastward; cloudless sky.
P.M.	1	35¼	12	29·997	—	The same as preceding hour.
	2	35	11	29·997	—	A bed of cirrostrat. forming in N.W. horizon. Otherwise the same.
	3	34½	12	30·006	W.	Still very calm; clouds a cirrocumulative appearance forming on the N. horizon. Sky otherwise clear.
	4	33	14	30·011	NWbyW	Gentle wind; the cirrocumulative clouds continue to form on the N. horizon, and are rising towards the zenith.
	5	34½	16	30·017	W.N.W.	The same as last hour.
	6	35	16	30·021	W.	Calm; the sky obscured by the cirro. clouds which, comm

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kept at the Manse of the Parish of Abbey St. Bathan's, Berwickshire, Lat. 55° 52′ N. Long. 2° 23′ W. at the height of about 450 feet above the sea. By the Rev. John Wallace.

DECEMBER.

DATE. 1835.	THERMOMETER. IX.A.M.	X.A.M.	III.P.M.	X.P.M.	HYGROMETER (Leslie's.) IX.A.M	X.A.M	III.P.M	X.P.M	Rain in Inches. Weekly.	BAROMETER. At 32° of Fahrenheit. IX.A.M.	X.A.M.	III.P.M.	X.P.M.	Direction of Wind at X.A.M.	REMARKS.
Tu. 1	45¼	45¼	42¼	43¼	2	3	4	0		28·631	28·631	28·658	28·692	S. S. E.	Calm, A.M. partially clear, P.M. fog and rain.
We. 2	43¼	43¼	41¼	34	4	10	10	3		28·672	28·672	28·830	28·990	W. N. W.	Gentle breeze. A.M. showery, in the evg. soft clouds.
Th. 3	38¼	38¼	40¼	38½	3	3	2	1		29·084	29·084	29·039	28·971	E. S. E.	Calm, A.M. clear, P.M. cloudy, evg. the sky overspread with fleecy clouds.
Fr. 4	40¼	41¼	41¼	39¼	7	10	10	13	1·562	29·005	29·030	29·072	29·163	NWbyW	Gentle breeze, sky clouded (cirro. S.W. prevalent), evg. rain with strong wind.
Full M. Sa. 5	34	35	35	33½	12	8	6	2		29·531	29·555	29·560	29·566	W.	Calm, morning clear & frosty, a circocumulus bed of clouds polarized E to W. [evening rain.
Su. 6	34½	34½	35	35	1	2	2	0		29·616	29·635	29·560	29·687	W.	Very calm, A.M. calm with dense fog, evening drizzling rain.
Mo. 7	35½	36¼	35½	38	0	0	1	1		29·698	29·698	29·641	29·640	W.	Very calm, A.M. dense fog, P.M. tendency to clear, evening cloudy.
Tu. 8	38½	38½	40½	38	4	4	4	8		29·482	29·469	29·301	29·244	S. E.	Gentle breeze, sky obscured by hazy clouds, evening rain.
We. 9	36¼	37	37½	34	15	20	19	7		29·353	29·563	29·625	29·749	S. E. W.	Calm, A.M. occasional showers, P.M. tendency to clear, but heavy masses of cld.
Th. 10	33½	33½	34½	34	11	12	7	7		29·768	29·570	29·732	29·610	S. S. W.	Gentle breeze, sky partially clear, evg. sky veiled by a thin haze, wind rising.
Fr. 11	33½	33½	36¼	43½	7	7	6	6	0·225	29·477	29·469	29·393	29·545	S. S. W.	A.M. brisk wind, P.M. strong wind, with hazy masses of cloud, evg. cloudy.
Sa. 12	31½	33	39	41½	3	4	4	4		29·691	29·687	29·665	29·627	W. S. W.	Calm, hoar frost morning, sky overspread with cirri. P.M. occasionally cloudy.
Su. 13	40½	41	45½	40	13	13	13	13		29·653	29·662	29·588	29·574	W. S. W.	A.M. calm, heavy masses of cloud prevalent, P.M. wind rising, evg. calm.
Mo. 14	44½	44½	45½	39	21	21	12	5		29·865	29·591	29·640	29·791	W. S. W.	Brisk wind, A.M. heavy clouds atrest an a blue sky, P.M. overcast, evg. calm.
Tu. 15	31½	33	35½	32½	5	6	4	0		29·862	29·886	29·880	29·858	W. S. W.	Calm, hoar frost morning, cirro-st. tending to the cymoid formation, evg. calm.
We. 16	32½	33½	36	37½	2	6	10	8		29·702	29·863	29·789	29·765	W.	Calm, overcast, P.M. cloudy.
Th. 17	43½	43½	45½	39½	16	16	20	13		29·567	29·702	29·656	29·656	W.	Calm, overcast, evening clear.
Fr. 18	38½	39½	43	39½	17	13	18	0	0·450	29·732	29·564	29·561	29·651	W. N. W.	Brisk wind with light clouds floating, at 5 P.M. violent gusts of wind with rain.
New M Sa. 19	36½	36	34	32	5	17	15	7		29·845	29·744	29·771	29·834	E.	Strong gusty wind, sky overcast, prevalent but slight showers.
Su. 20	33	33	33½	34	4	5	5	12		29·861	29·849	29·829	29·845	N. W.	Calm interrupted by occasional gusts of wind, sky overcast and lowering.
Mo. 21	34½	35	34½	36½	15	15	17	14		28·865	29·869	29·869	29·925	N. W.	Calm, A.M. overcast, P.M. drizzling rain.
Tu. 22	30½	35½	34½	35½	14	13	9	11		29·996	29·997	30·006	30·040	SW by W	Calm, clear with intense frost, evening cloudy.
We. 23	34½	35½	37½	37	17	13	5	5		30·047	30·033	30·002	29·984	N. W. N. W.	Gentle wind, sky overspread with small detached clouds, light haze.
Th. 24	34	34	31½	29½	3	9	6	5	0·051	29·925	29·908	29·883	29·846	NWbyW	Gentle wind, A.M. white clouds scattered over the sky, P.M. cloudless.
Fr. 25	33½	33½	34½	34½	3	3	3	1		29·808	29·808	29·799	29·782	NWbyW	Calm, A.M. light haze, P.M. nearly cloudless with a fine blue sky.
Sa. 26	34½	34½	35½	40	6	4	5	7		29·703	29·694	29·540	29·480	NWbyW	Brisk wind, A.M. overcast with light drizzling rain.
Su. 27	40½	42½	43½	43½	8	5	16	15		29·446	29·443	29·406	29·343	S. W.	Brisk wind, A.M. overcast with tendency to rain, P.M. light showers.
Mo. 28	44½	44½	40½	38	14	13	13	5		29·046	29·016	29·096	29·264	W. by N.	Strong wind, A.M. overcast with showers, P.M. clouds dispersing, evg. clear.
Tu. 29	38½	38½	40	35	16	9	9	0	0·125	29·469	29·509	29·096	29·592	W.N.W.	Gentle wind, fine, cumuli prevalent.
We. 30	37	37	35	30	6	6	15	15		29·614	29·464	29·681	29·743	W.N.W.	Gentle wind, light rain morning, clearing towards noon, evg. intense frost.
Th. 31	25½	27½	33½	32½	2	3	0	0		29·715	29·699	29·682	29·682	N. E.	Calm, A.M. clear, P.M. overcast and fog with tendency to rain.
Means	36·4	37	37·9	36·6	7	8	9	6	9·4	29·569	29·572	29·561	29·585		

Means for the Month — { Therm. 36·8 } { Mean temperature of spring water 46°¾ }
{ Hygr. 7. } { Mean point of deposition 32°. }
{ Bar. 29·579 in. } { Moisture in a cubic inch of air = ·001349 grs. }

RECORDS

OF

GENERAL SCIENCE.

ARTICLE I.

Memoir of John Napier, Baron of Merchiston.
By J. B. BIOT.*

(Concluded from p. 94.)

HITHERTO, we have only seen Napier as a Scotch Baron of
the 16th century. Confined to the heart of a barbarous
country, in a strong castle, he lived in a solitary manner
with his family, without any other change in his thoughts,
than that which the administration of his affairs, or his un-
avoidable participation in religious and political quarrels
demanded. A decided and ardent Presbyterian, he com-
mented on Scripture after the manner of his times, and
under the influence of the same prejudices which inflamed
the other fanatics of his sect, he explained, with a confi-
dence not less determined than blind, the allusions of Holy
Scripture as referring to the circumstances in which the
reformed church was then placed. Yet to this period we
owe his invention—for it should not be termed discovery;
an invention almost mechanical and material, which
changed all the methods of mathematical calculation, till
then, employed in science, gave them a wonderful simplicity
and precision, and saved so much labour to laborious men,
as well as those of genius, such as Copernicus and Kepler,
that it is but proper to show to inquiring minds how Napier
produced such an admirable invention.

The Scotch biographer felt the want of such a view; but

* Abridged from the Journal de Savants, May, 1835.

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he could derive little assistance from the writings of mathematicians, and even from those who have written particularly on the history of the mathematics; for, by a fatality almost inevitably attached to inventors whose discoveries are subsequently improved by the succeeding progress which they excite, the original work of Napier entitled, " *Mirifici Logarithmorum Canonis Descriptio,*" published in 1614, is never read, although he explains in it the mode of generation which he attributed to the new quantities called by him *artificial numbers,* or *logarithms,* to which he joins their numerical properties, exhibiting their use in simplifying arithmetical calculations, when it is necessary to multiply numbers by each other, or to divide the one by the other, as well as their employment in the determinations of trigonometry and astronomy, and, lastly, the numerical tables containing the logarithms of trigonometrical lines called *sines, co-sines, tangents,* and *secants,* calculated from minute to minute for every degree on the quarter of the circle, which, without the employment of the invention would cost enormous labour. All this is given without any explanation, without any insight into the ideas which had led him to conceive the admirable utility of these tables, nor upon the means which he employed in calculating them. His work entitled, "*Mirifici Logarithmorum Canonis Constructio*" is also no longer read. It was published after his death, by his son, in 1619. In it he explains and demonstrates all the processes, all the mechanism in the construction of his *logarithmic tables,* which he did not wish to develope at first. We do not require at the present day a knowledge of his original ideas, nor of his method. The immense developement given to algebraic calculation, by the use of letters as symbols, of which, the introduction is due to Vieta, has furnished us with rapidly, indefinitely, converging series, by means of which, we attain the same logarithms in a direct way, almost without labour, with a neatness of symbols which permits us always to see the present effect of general operations which we express by formulæ, and enables us to appreciate the degree of the approximation of our results. Although the precision to which they may be pushed is without limit, still it must be asserted to the honour of Napier, that the same advantage is connected

with his method; and, if this assertion should appear too much strained to modern analysts, I hope to be able shortly to afford such proofs as will overcome their objections.

But, in order to have a just notion of the work of Napier, it is necessary to study it from these books, especially in the second, where he explains his method; and not to trust to the abstracts which have been given by authors. Of all these abstracts, the best, that is to say, the most faithful and elaborate is, in my opinion, that published by Hutton in his introduction to the mathematical tables of Sherwinn, and which is re-printed with this introduction in the first volume of the " *Scriptores Logarithmici.*" The path followed by Napier is there strictly followed, such as it is characterized in his principle, and appreciated in his results, comparatively with our actual methods. Now, this is what we wish to know of a first inventor. Montucla, the historian of the Mathematics, we should almost be tempted to believe, had never inspected the posthumous work of Napier, for he attributes to him methods of bi-section which were not his, and which were employed afterwards by Briggs. We might expect to find a more just estimate in the history of Astronomy by Delambre, who was neither deficient in a knowlege of the actual logarithmic methods, nor in the love of truth. But, by a defect in philosophy, which is but too remarkable in his work, he employs not only the simplicity of our modern formulæ to exhibit the ideas of Napier —what should be their true use, but translates imperfectly these ideas into modern formulæ, and thus gives them as a basis, an empirical approximation which they do not possess, and which is positively opposed to the spirit of Napier's method. Thus disfigured, he examines the latter, demands an explanation for want of exactness which Napier has not committed, and for faults which he attributes to him from his own errors. The new Scotch biographer endeavours to rescue the honour of Napier from the criticisms of this author, and opposes, successfully, the small number of writers, especially English, who, from a sincere scientific opinion, or from national prejudice have, according to him, endeavoured to depreciate Scotland, by attributing the first idea of the discovery of logarithms to an obscure mathematician of the continent, called Justus Byrge, of whom Kepler

speaks in his introduction to the Rudolphine tables, as
having thought of something of the same kind, without
publishing it.* But what is the use of discussing preten-
sions which have not been produced, and which no one at
present can see or appreciate?

At the time when Napier invented the logarithms, all
mathematicians, all astronomers, and they were then very
numerous, felt the want of some method which would sim-
plify the prodigious calculations which were necessary for
the resolution of the celestial triangles, the only application
of mathematics which was then known. Different scientific
details attest the trials made in this respect by Byrge, as
well undoubtedly as by many others, among whom Kepler
himself may be mentioned; and, in short, when we think
of what must be the numerical calculation of the tables of
natural sines and tangents, for a radius expressed by a mil-
lion, or even by ten millions of parts as they were then con-
structed; when we consider that all this required constant
divisions and multiplications which were required to be
accurately made, without omitting a single cypher from
the largest numbers, we can easily understand that the
desires of mathematicians would be directed to the object
of freeing themselves from such drudgery, and, that neces-
sity would suggest numerous methods for attaining this
desirable end. To these titles his right of inventor, but of
inventor only, is incontestible. But this right becomes, if
possible, more clear still, when we study the principle of
his tables, when we expose their basis, when we understand
the originality, and appreciate the justice with which he
applies it, and the precision of the results which he deduces.
If I can restore what commentators have consigned to ob-
livion, I may say what Cicero says of Archimedes : "*Humi-
lem homunculum e radio et pulvere excitabo.*" But I shall
then have given subject of satisfaction to philosophers who
love the glory of their predecessors as their heritage, and
are happy when justice is rendered to their works.

It was the great genius of Syracuse, in his treatise "*De*

* The words are "Qui tamen apices logistici Justo Byrgio multis annis ante
editionem neperianam viam præiverunt ad ipsos logarithmos. Etsi homo cunctator
et secretorum suorum custos fœtum in partu destituit, non ad usus publicos
educavit.—(*Tab. Rud.* iii. p. 11 in fol.)

Arena," who first exhibited the properties of progressive
numbers upon which the theory of logarithms is founded.
Archimedes had proposed, not the idle question of how
many grains of sand could be contained in a sphere equal
in diameter to the sphere of the stars, such as they were
then supposed ; but to show that a number so great, and
even infinitely greater might be specified and written with
the characters alone of numeration used in his time among
the Greeks.   We know that these characters were letters
of the alphabet, which were employed consecutively in
their natural order, either simple or accented, to express
the different unions of units, tens, hundreds, until they
arrived at ten thousand units, which were called a *myriad*,
and which was designed by the capital letter M surmounted
by the alphabetic letter which expressed the number of
myriads to be noted.   In addition to this, Archimedes con-
ceived an indefinite series of numbers commencing with
simple unity, and sucessively decupled the one by the other
in such a way that they were written in the order of our
notation, as

$$1 ; 10 ; 100 ; 10000 ; 100000 ; \&c.$$

But, as in writing them thus we are soon greatly embarassed
by the great number of cyphers which follow unity, we
abridge their expression by the assistance of an ingenious
method contrived by Descartes, and which consists in writing
only their common factor 10 affected by a numerical indicator
more or less considerable, which marks—which *exposes* how
many times the common radical 10 is found multiplied by
itself.   Then, in writing thus, the successive terms of our
progression, and marking below each term the rank which
it occupies after the first, we obtain the following lines,
$1 ; 10^1 ; 10^2 ; 10^3 ; 10^4 ; 10^5 ; 10^6 ; 10^7 ; 10^8 ; \&c.$ indefinitely.
$0 \quad 1 \quad 2 \quad 3 \quad 4 \quad 5 \quad 6 \quad 7 \quad 8$
It becomes evident at first sight that the number of the in-
ferior line, which expresses the rank of each term, is equal
to the exponent, which indicates the number of times the
common radical 10 is a factor in this term.   This was not
distinct to the eye in the notation by letters employed by
Archimedes, and even, it was not possible for him to ex-
press, as we do, the character of indefinite extension, which
he wished to give to such a succession.

Then, what did he do? He considered first, the nine first terms from 1 to 10^8 separately. Now, he can write them, and even name them, since the last term 10^8 or ten times ten thousand only was equal to a myriad of myriads; separating, then, the first 8 terms, he called them *numbers of the first order;* then, with the ninth term 10^8 he forms a new unit, which he terms *of the second order;* and he arranges these new units, like the preceding, in numbers progressively decupled the one by the other, until he arrives at the 8th term, which is 10^{15}; so that the following 10^{16} is found to be a myriad of myriads of numbers of the second order. Working with this term 10^{16} as with 10^8 he forms a new unit, which he terms of *the third order;* and, continuing to form successive numbers of units, each of which commences at the myriad of myriads of the preceding numbers, it is evident that we may proceed in the series as far as we please, and even express all these terms orally, for it is sufficient to consider them all placed consecutively, and to separate them by orders or octades, as in the following lines,

1st Order,
$$1; 10^1; 10^2; 10^3; 10^4; 10^5; 10^6; 10^7.$$
2d Order,
$$10^8; 10^9; 10^{10}; 10^{11}; 10^{12}; 10^{13}; 10^{14}; 10^{15}.$$
3d Order,
$$10^{16}; 16^{17}; 10^{18}; 10^{19}; 10^{20}; 10^{21}; 10^{22}; 10^{23}.$$
4th Order,
$$10^{24}; 10^{25}; 10^{26}; 10^{27}; 10^{28}; 10^{29}; 10^{30}; 10^{31}; 10^{32}, \&c.$$

Thus, any term, however far distant from the first, may be completely defined and named, by stating the order or octade to which it belongs, with its place in the octade; and besides, this method of characterizing it will be infinitely more simple, than if we wished to write it in an explicit manner; for example, in proceeding from the dimensions of a small grain of sand, and raising it from multiple to multiple by means of his series, until we conceive it to fill a sphere of an equal diameter to that of the stars, Archimedes proves that the total number of these grains, will be less than a thousand myriads of 8th numbers; now, according to this table, it is easy to see that the simple units of this 8th order have for their expression the

number 10 multiplied 56 times in succession by itself; and
as a thousand myriads of units make a thousand times ten
thousand, or 10 seven times factor, we see that the number
announced by Archimedes is equal to 10 multiplied by itself
63 times, which, even with our Arabic notation, would be
tedious to write, since it is unity followed by 63 cyphers.
But the matter becomes very simple, more simple even for us
than for Archimedes, if we employ the notation of the ex-
ponents of Descartes, which expresses only the number of
times the multiplication of 10 by itself ought to be per-
formed, for then the immense number of Archimedes would
be expressed in this small abridged formula, 10^{63}.

In all this, the simplicity results from the fact, that in
place of considering the same numbers with the multiplicity
of characters which express them, they are only named by
their rank in the indefinite progression. In following out
this idea, Archimedes proves that it serves equally for ob-
taining the products of the terms of the progression among
them in the simplest manner. For, suppose that we wish
to multiply the fourth term, which is 1000 or 10^3, by the
fifth, which is 10,000 or 10^4, the product will be 10,000,000
or 10^7; but, in place of writing all the characters which
compose them, it is sufficient to add together the figures 3
and 4, which express or *expose* the rank of the terms which
have been multiplied. For the sum 7 marks the number
of times that 10 is the factor in the product sought, and
it is sufficient to write the product 10^7. The multiplication
is thus re-placed by addition, which is a much more simple
operation.

Inversely, if you have the product 10,000,000 or 10^7,
which is one of the terms of the series, and you wish to
divide it by 1000 or 10^3, which is another term, you have
only to take the difference of the exponents, which is 7 less
3, or 4, and 10,000 or 10^4 will be the quotient sought, the
same as would be obtained by the longer process of division.
All the other terms of the series present the same facilities
for abbreviation, when they are multiplied or divided by
themselves, which results from the circumstance, that they
successively derive the one from the other in sequence, a
similar proportion, forming thus what is called *geometrical
progression* or by quotients, while the simpler numbers,

which express only the rank of each term, increase only by one unit, and always by one unit in passing from one term to the following term, which constitutes another kind of progression called *progression* by *equi-difference*, or *arithmetical*. Archimedes recognized and proved all that we have explained respecting the relations of two similar progressions, when these terms were thus placed in correspondence. And, in order to show that these properties took place for any terms of the two series, he thought of representing generally these terms by letters employed only as signs of quantities without any peculiar numerical value, giving thus the first example of reasoning applied to figured symbols, representing abstractions in which algebra consists, that powerful instrument of the mind for discovering the general relations of great sums.

From this to logarithms there is only a step, and even logarithms are only indices employed after the manner of Archimedes to express the rank of each number in an indefinite geometrical series which comprises them all; so that their multiplication and division with each other may be re-placed by addition, or the mutual subtraction of indices which correspond with them. But how can we comprehend all the numbers in the same geometrical series, proceeding continually by equal proportions? It is in this that the fundamental idea of Napier consists. To make this proportion common, if near equality, it is necessary only that the progression should proceed by very slow steps, so that any number given, if it does not fall upon one of the terms of the progression, is found at least comprised between two terms differing so little from each other, that the error may be neglected; or better still, it is only necessary to represent, as Napier did, geometrical progression and the corresponding arithmetical progression, as produced by the continued motion of two moving bodies proceeding from a state of rest, and advancing, the one with a geometrical acceleration, the other with an equi-different and uniform motion. The simultaneous positions of the two moving bodies at any time will give in geometrical progression the number, in arithmetical progression the index or the logarithm which corresponds to it.

But this idea, simple though it is, presents, in the execu-

tion, a very great and material difficulty. To form the successive terms of the geometrical proportion, it is necessary to multiply them successively by their common proportions, as many times as there are units in the index of their rank. Hence, we fall into the calculations of multiplication, which we wished to avoid. Napier relieves us from the difficulty by a very simple method; he forms his geometrical progression by descending from the greater to the smaller number, instead of rising from the small to the great as Archimedes did, and he employs for the constant proportion of the successive terms that of 10 to 9, or of 100 to 99, or of 1000 to 999, or generally, by an entire power of 10 to this same power diminished by a unit. Then each term may be deduced from the preceding by simple subtraction, for if the first term is, for example, 10000000, and the second 9999999, the latter will be obtained by subtracting unity from the former, which is the millionth part. The third will be deduced from the second in taking from the latter the ten millionth of its value, or 0·9999999, according to our actual decimal notation, and in continuing this proceeding we obtain as many terms as we wish by simple subtraction, which will follow in the same geometrical proportion as we shall have chosen. The correspondence of the terms and indices which mark their rank will form the following table, in which the succession is indicated to the hundredth term after the first, carrying the value of each term to the seventh decimal place.

Index of the rank of the terms of the geometrical progression departing from the first.	Numerical value of the successive terms of the geometrical progression.
0	100000000·0000000
	1·0000000
1	9999999·0000000
	0·999999
2	9999998·0000001
	0·9999998
3	9999997·0000003
	0·9999997
4	9999996·0000006

and so on to the 100th term which will be

| 100 . . . . | 9999900·0004950 |

This is precisely the first table formed by Napier, which
has been copied for the purpose of giving an accurate idea
of his method. We may apply to the terms which comprise it
all the properties demonstrated by Archimedes in reference
to geometrical progression, and obtain the same simpli-
fications by multiplying and dividing them by each other.
But, however slow the progression here employed may be,
it is still only the expression of an intermittent movement,
while the definition of the logarithm requires us to deter-
mine the indices of the rank which shall correspond to the
same terms produced by a motion completely continued.
Napier did not obtain the absolute expression for this cor-
rection, as we now can do by our differential methods,
which enables us to pass, without error, from intermission
to continuity.

But, in comparing the essential conditions of continued
motion with those of intermittent motion, he established
measurable limits between which the logarithm of a given
number is always comprised; so that if these two limits
exceed the order of the decimals which are to be preserved,
we may legitimately take any one, or, what is still better,
their mean, as a sufficiently near approximation to the
logarithm. Applying this to his table, he shews that the
logarithm of the first term 9999999 is necessarily com-
prised between 1,0000000 and 1,0000001, so that it becomes
equal to 1,00000005; now, the exact value of the logarithm
calculated by our actual methods is 1,00000 00500 00003
333, so that the valuation of Napier only errs by one-third
of unity above the 14th decimal of this logarithm. It is then
the first term of the arithmetical progression corresponding
to the geometrical progression which he has adopted; then
by multiplying by the successive numbers 1, 2, 3, &c. which
mark the rank consecutively of the terms of this geometri-
cal progression, we obtain the indices, that is to say, the
logarithms of all these terms. It is in this way that he
operates, and with some abbreviations carries on his table of
correspondence from 10000000 to 5000000, where its pro-
gressive decrease indicates the proportion of 2 to 1. Then,
if we point out any number comprised between these limits,
he shews how we may obtain its logarithm with the requi-
site approximation, by comparing it with the two terms of

the geometrical progression between which it is included.
If the proposed number exceeds the limits of the table, he
shews how we may obtain the logarithm by causing it to
re-enter.

The general problem, therefore, of determining all num-
bers, exactly or approximately, in the same geometrical
progression is thus completely resolved ; and then, by mul-
tiplying and dividing these numbers the one by the other,
whatever they may be, we obtain the same facilities as
Archimedes had found for the particular geometrical pro-
gression of which he has made use in his work on the num-
ber of the sand. Such is the invention of Napier. He has
rendered continuous and general for all numbers the ad-
vantages which Archimedes had only obtained for inter-
mittent and particular numbers. If we ask why Archimedes
did not attain this second step, which now to us appears so
near the first, we may find, in our opinion, a plausible
reason in the nature of the symbolic letters employed in his
time to distinguish the numbers. For the signification of
these characters being absolute, numbers differing very
little from each other were often expressed by characters
which had no apparent relation to each other, or, if their
expressions possessed common elements, the proportion
of the size of the latter to similar ones was not indicated
by the same numerical expression ; in place of these two
kinds of indication existing and striking, so to speak, our
views in our actual mode of writing the number ; above all,
when generalizing the idea which gives a value of position
to the cyphers, we advance in an inverse sense to the sub-
divisions of units, by the employment of decimal cyphers.
Hence, this is one of the examples of the influence of signs
upon the extension of ideas with which the history of ma-
thematics abounds.

We may remark on this subject, that Napier first em-
ployed, in Europe, this generalization so simple in the
mode of writing decimal sub-divisions, which was indispen-
sible to produce successive subtractions. If we wish to con-
vince ourselves that this idea was not so easy to discover,
as we might think at present, when it has become familiar
to us by use, we have only to inspect the complicated and
almost impracticable means by which Steven, an able and

ingenious geometrician, attempted, a short time before, to write decimals. In fact, Pitiscus substituted actual notation in 1612, in the second edition of his trigonometry; and the "*Canon Mirificus,*" where Napier employs this notation, did not appear till 1614; so that Pitiscus deserves the credit of priority in publication. But, that Napier, who employs it constantly in his tables, has contrived it independently of Pitiscus, appears incontestible, when we consider the number of years which the calculation of those tables must have required; and, thus is proved their prior use probably long before Pitiscus, who did not employ them in his former edition, in 1599.

The system of logarithms adopted by Napier was the the most simple, and most convenient which could then be conceived, for forming the successive terms of the geometrical progression. The tables which he had constructed presented immense advantages in regard to simplicity, as has been already explained for multiplication and division. Kepler adopted them, and published a copy in his Rudolphine tables, of which, as has been stated, he changed the plan in order to adapt them to the use of the logarithms.

But, when the invention is considered, we can readily see that the logarithmic system of Napier was not that which was best fitted for our decimal mode of numeration. Briggs, Professor at Oxford, a contemporary of Napier, proposed another which offered this advantage, and which is that used at present; he appears to have received the notion from Napier himself, whom he visited several times in Scotland. At the end of the posthumous work of Napier there is an appendix, in which the method employed by Briggs is pointed out. However it may be, Briggs constructed, with ability, on this new system, excellent tables, the most accurate, and the most extensive which had then been published. It is a work characterized by great patience of calculation, and even claims ingenuity from its numerical approximation. But some consider themselves authorized to attribute, on this account, to Briggs a share in the invention. This is, however, to confound merits totally dissimilar, viz. genius and labour. But the lively passion for discovery is not a vulgar faculty, and it is too often re-placed by one less honourable, viz. the secret desire of second rate minds to pull down that which is elevated.

Besides the merit of the invention, the tables of Napier
are a prodigy of patient labour. When we think of the
time and labour required to calculate these tables, we are
led to be anxious lest any chance should have prevented
him from realizing his idea, and that it should have died
with him. It has been said, and Delambre has repeated
the statement, that the last cyphers of his numbers were
incorrect. This is true, but a more useful fact would have
been, to know if the error arose from the method, or from
some fault in the calculation. I have done this, and have
ascertained that it was in reality produced by a fault of the
latter kind, a very small error in the last term of the second
progression which he forms for preparing the calculation of
his table. Now, all the following steps are deduced from
this which produces the small error remarked. I have
corrected the error, and *with his method*, but abridging the
operations by our more rapid processes of developement,
have calculated the logarithm of 5000000, which is the
last of the table of Napier, upon which, consequently, all the
errors accumulate; I have found its value 6931471808942,
while, by the modern series, it ought to be 6931471805599.
Thus, the difference begins at the tenth figure. I have
calculated, likewise, the hyperbolic logarithm from 10
according to the corrected numbers of Napier; I have found
for its value 2,30258 50940 346, while, by our actual tables,
it is 2,30258 50929 940; the real difference then takes
place at the ninth decimal. If Napier had possessed at his
disposal a village teacher to calculate, by subtraction, a
geometrical progression more slow still than that of which
he has made use, the tables of Briggs with 14 decimals
would not have been superior to his.

After this immense invention of logarithms, we can
scarcely mention some other works, which proceeded from
him. The former was sufficient for his life and for his
glory. He discovered some ingenious theorems for con-
tracting, in certain cases, the resolution of spherical tri-
angles, and these have been called from his name *the ana-
logies of Napier*. But their utility was greater in his time
than at present. The advancement of analytical processes
has superseded, in a great measure, the use of particular
reductions; and we now understand, that these general

methods are also the simplest. He also planned for abridg-
ing common calculations a small piece of mechanism com-
posed of rulers divided by squares, in which the products of
the first natural numbers were written; this was called
Napier's rods or *bones*. Its application to common calcula-
tion is even very limited. Lastly, the author of the new
biography has given some extracts of numerical or alge-
braical researches found among his papers; but they pre-
sent, in our opinion, only two points worthy of remark.
The first is, that Napier had formed perfectly clear notions
of the calculation of decimal fractions, and of the appre-
ciation of irrational quantities by numerical valuations,
more and more approaching the true value, without always
being able to express them exactly in finite numbers. The
second peculiarity is, that in studying the elevation of
numbers to their different powers, we see that he had re-
cognized the triangular form under which the co-efficients of
the entire binomial powers are arranged, when they are
placed consecutively one below the other in their consecu-
tive powers, a remark, which, it was considered, belonged
to Pascal, who used it, like Napier, for the elevation of
powers, and the extraction of roots. But Pascal did not
publish it till 1665; so that in reference to the invention,
at least, Napier preceded him.

The Scotch biographer concludes from this, that if Napier
had lived longer, he probably would have discovered before
Newton the Binomial theorem, or even the differential
calculus, and he pushes this idea of superiority so far
that he makes the remark, that Newton having never
been married could devote all his life to intellectual labour,
while the Scotch philosopher, as he calls him, had two
wives and 12 children. We think it sufficient for Napier
to have discovered the logarithms. But we shall finish this
abstract by a curious enough approximation. We have
often occasion to observe in literary history, that the same
inventions, the same discoveries, with slight shades of dif-
ference, present themselves at the same time to several dis-
tinguished minds, without any communication between
them. It proceeds from the circumstance, that these new
ideas are, so to speak, prepared and produced by the dis-
cussion of preceding ideas, from which they are derived.

The simultaneous application to the same subject alone
ought to produce, for the strongest reasons, inventions at
the same time, or rather, of perceptions of the consequences
which present themselves mechanically as it were. Such
is the arithmetical triangle which refers to the powers of
numbers; and this is so true, that from a remark which
has been communicated to me by my son, and of which I
append the proofs in a note which he has sent me, the
arithmetical triangle, with its use for the elevation to entire
powers, and for the extraction of their roots, is cited as a
very ancient invention in a work printed in China, in 1593,
when the Jesuits could only reach Canton with difficulty,
and were not acquainted with the arithmetical triangle, as
it was only discovered in Europe 60 years after. We see
from the same work, that the Chinese had been conducted,
at a very early period, to the principle of properties of
figures, to the summation of the series of natural numbers,
of their squares, and to different other properties which
have been discovered more slowly in Europe, probably by
a train of the same ideas. The author of the note remarks,
" that the formation of the binomial for entire powers
existed with the Arabs in 1430, who appear to have learned
it from the Hindoos ;" and he adds, " that the same notions
contained in the Chinese work bear marks of this origin,
from the fact, that among the different orders of numerical
units, which are all decimals, those of very high orders
are designated by the term of ' *sands of the river Ganges.*' "

These details of literary history have appeared sufficiently
curious of themselves, and besides, as connected with the
work of Napier and Pascal, in the invention of the arith-
metical triangle, to deserve a place here.   Biot.

Note.—The Royal Library contains, in the Chinese col-
lection, two copies of a work entitled, ' *Souang-Fa-Tong-
Tsong,*' or the principles of the art of calculation; which,
according to the preface, was printed in 1593, under the
emperor Wan-ly, of the dynasty of Ming, who reigned in
China afer the expulsion of the Mongols. This work,
divided into several parts, contains a Chinese treatise on
arithmetic, a book upon geometrical figures, and upon
the principal figures of surveying, upon the extraction

of powers, and the extraction of roots ; and lastly, several
books upon different questions applicable to commercial
transactions. These questions extend from the rule of
three to the summation of very complicated numerical
series, such as the series of squares, of natural numbers,
and that of triangular numbers.

At the third page of the book upon the extraction of
roots, (6 sect.) we find a table entitled, ' *Table which gives
the method of finding the union'* (*lien* angles), a term used in
the work, to indicate the co-efficients of the developement
of any power of a binomial. This table is disposed exactly
like the arithmetical triangle of Pascal, and presents the
successive series of powers of 1 + 1 from the power 1 up to 6.
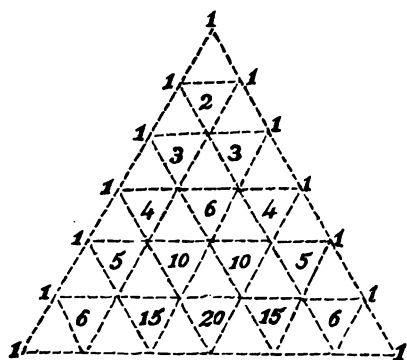
Origin of successive numbers.

Root extracted.

Equal figure or figure for one mul-
tiplication.

Solid figure or figure for two mul-
tiplications.

Figure for three multiplications.

Figure for four multiplications.

Figure for five multiplications.

The exterior line to the left, says the text, contains
the numbers *tsi*, (the numbers which are to be added). The
exterior line to the right contains the numbers *yu*, (coins).
*It is the expression employed in the work to express the last
term of a binomial raised to a power.* The numbers which are
placed in the middle of the others form the *union* (angles
or co-efficients). With this union, we have the true process
for extracting the roots of powers.

Disposition of the table.—In proceeding from the top, we
observe 2 which forms the sign of the equal figure (*phing-
fang*). We observe then 3 and 3, which are the signs
of the right or solid figure (*li-fang*). Then we have 4, 6, 4,
which are the signs of the figure for three multiplications

(*san-ching-fang.**) In continuing to the end, we may deduce the figures of 30 multiplications, and even more. Such is the excellence of this method. Here we stop at the figure of 5 multiplications. By imitating it, we may obtain all the degrees of co-efficients sought.

A note placed at the side of the triangle indicates, that this table was already given in a more ancient work (the collection of *Ou-Chi*), but without an explanation of the method of forming it.

The notation of the powers is made, as we see, by a regular method, which points out how many times the root is multiplied by itself.

The power 1 is called the root.

,,   2   ,,   the equal *figure*, the *figure for one multiplication*, that is to say, as the text expresses it, the figure where we multiply the root by itself.

,,   3   ,,   the right or solid figure, that is, *the figure for two multiplications*, or where we multiply the root twice by itself.

,,   4   ,,   *the figure for three multiplications*, or where we multiply the root three times by itself.

,,   5   ,,   *the figure for four multiplications.*

Thus, the power of the order *m* will be the figure for *m* — 1 multiplications.

The author explains, that the two first powers alone are represented by figures, and that the others are only the result of numerical operations.

In employing the co-efficients given by the table, several square and cube roots are extracted in the same work. There is even an example of the extraction of a root of the 4th power, which is effected by employing the co-efficients 4, 6, 4, given by the table. In relation to this last case, the author adds, that it is more simple to extract first the

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* Such is the orthography of the Chinese second and third numerals, (*li, san*) in the French version. The common pronunciation, even by the best educated Chinese I have met with is, *yee* (two,) *samm* (three). Perhaps, in the first, the liquid *ll* might convey the sound of *y*.—EDIT.

square root of the number given, and then the square root of the number found.

From what precedes, we see that the formation of co-efficients of the different powers of a binomial expressed in whole numbers, was known to the Chinese, at least, in 1593. At this period, the first Jesuits, as missionaries, arrived in China; but the arithmetical triangle of Pascal was not published in Europe till 1665. And, besides, if we examine the nature of the questions treated of in the work cited, and the methods there given for resolving them; if we consider the squares and magic circles which it contains, we cannot believe that any European had assisted in digesting it.

The theorem of the formation of the co-efficients of a binomial existed with the Arabs in 1430, and, perhaps, was unknown in China, till after the conquest of the Mongols, who called some Arabian philosophers to their court. We find, even in the last section of the Chinese work, the mode of multiplication, by triangular network, adopted for a long time by the Arabs; and again, in the classification of different numerical units, the units of a very high order are distinguished by the name of '*sands of the river Ganges*,' which indicates some prior relations of the Chinese with the Hindoos, who had also, as we know, notions of algebraical science and geometry.

From the same Chinese work, we see that the Chinese, at this period, knew the theory of similar triangles, the exact measure of the pyramid and cone, as well as that of the mass of the cone and pyramid, and the proportion $\frac{22}{7}$ of the circumference to the diameter, as they generally used in these calculations the proportion of 3 to 1. Besides the summation of series which we have noticed, they understood the resolution of equations of the second degree with one unknown number; they even resolved by groping, numerical equations of the third degree with one unknown number, of which, it is true, they only extracted one root. But, neither in this work, nor in those of the Arabs and Hindoos, do we find notation by letters employed symbolically to express numerical quantities as we do at present. This invention, which constitutes truly the strength of algebra, is altogether European, and due to Vieta.

<div align="right">EDOUARD BIOT.</div>

Article II.

Observations on the Atomic Weights of Bodies. By Thomas
Thomson, M. D., F. R. S. L. & E., Regius Professor of
Chemistry in the University of Glasgow.

When we attempt to establish the atomic weights of bodies
with precision, it is obvious, that the first step must be an
accurate knowledge of the specific gravity of oxygen gas,
because an error in it will affect the whole of our subsequent
determinations, and prevent us from perceiving the beauti-
ful simplicity which nature has followed in establishing
these atomic quantities. Atmospheric air, it is well known,
consists, in a great measure, of oxygen and azotic gases;
which, as was first shewn by Mr. Cavendish, exist always
in the atmosphere in the very same proportions. But there
are constantly present in it carbonic acid gas, and the
vapour of water; the amount of both of which, especially
of the latter, varies considerably. Unless, therefore, we
take the precaution to free common air from all admixture
of carbonic acid and vapour before we attempt to analyze
it, we cannot expect to obtain results which agree ac-
curately with each other. I suspect that few experimenters,
who turned their attention to the constitution of atmos-
pherical air, have been at any trouble, either to dry it, or
to free it from carbonic acid gas, before subjecting it to
analysis.

The first person who shewed that atmospherical air does
not vary in its constitution (abstracting the carbonic acid and
vapour which are constantly varying), was Mr. Cavendish,
(*Phil. Trans.*, 1783, p. 106.) During the last half of the
year 1781, he tried the air of near 60 different days, to find
whether the proportion of oxygen was sensibly greater at one
time than another; but found no difference that he could be
sure of; though the wind and weather, on those days, were
very various: some of them being very clear and fair,
others very wet, and others very foggy. He tried the air
at different times of the same day, without finding any
alterations in the proportions of its constituents. He also
made several trials with a view to determine whether there

was any difference between the air of London and the country, by filling bottles with air on the same day, and nearly at the same hour, at Marlborough-street, and at Kensington; but the difference between them was never more than might proceed from the error of the experiment. And, by taking a mean of all, there did not appear to be any difference between them.

These important experiments and conclusions of Mr. Cavendish were forgotten or disregarded by chemists, till nearly the beginning of the present century, when Berthollet announced, that he had examined the atmospherical air in Egypt, and had always found it composed of about 79 volumes of azotic, and 21 volumes of oxygen gas. Sir Humphrey Davy made the same observations somewhat later, of the atmosphere in the neighbourhood of Bristol, and from the coast of Guinea; and in the year 1801, I ascertained, that the composition of the air, at Edinburgh, was precisely similar in its constitution. In consequence of the knowledge of these and some other facts of the same kind, chemists unanimously adopted the conclusion of Cavendish, that the composition of common air is constant. Hence, the inference, that it is a chemical compound of oxygen and azote is unavoidable.

In 1803, an elaborate set of experiments was published by Humboldt and Gay Lussac (*Ann. de Chimie.*, liii. 251), on the method of analyzing mixtures of oxygen and inflammable gases, by means of Volta's eudiometer, and, among other conclusions, they affirmed, that common air (abstracting its impurities) is composed of 79 volumes of azotic and 21 volumes of oxygen gases; the very same proportions which had been already adopted by Berthollet and Davy.

In the year 1808, Gay Lussac read a paper to the Society of Arcueil, which was published in the second volume of the Memoirs of that Society, proving, that gases always combine either in equal volumes, or one volume of the one with two volumes, or with three volumes, or with four volumes of the other, and never in any other ratios. This important conclusion was called in question by Mr. Dalton; but has been acquiesced in by all other chemists, and has for many years been adopted as a fundamental principle in chemistry.

If air be a combination of oxygen and azote, and, if this principle of Gay Lussac be true, it is clear that it cannot be composed of 79 volumes azote and 21 oxygen, because 79 is not a simple multiple of 21. And, if air be not a chemical compound, it is difficult to conceive how its composition should never vary under any circumstances whatever. Air, in this respect, is precisely similar to water, which is always a compound of one volume of oxygen and two volumes of hydrogen. But water is admitted on all hands to be a chemical compound.

A very slight alteration in the estimate of the constituents of air would bring it under Gay Lussac's law. Were we to consider it as a compound of 80 volumes of azotic and 20 of oxygen gas, it would consist of four volumes of azotic and one volume of oxygen gases, or of two atoms of azote and one atom of oxygen. It is difficult to avoid suspecting that a coincidence so very near does not hold exactly.

In the year 1803, when the experiments of Humboldt and Gay Lussac were made, chemical experimenting had not reached that degree of precision which it has now attained. And, whoever is acquainted with Volta's eudiometer, by means of which their results were obtained, and will take the trouble to read Mr. Cavendish's observations on eudiometers in the paper already referred to, will at once admit that a greater error than $\frac{1}{80}$th might easily be committed, even by very careful experimenters.

Aware of these sources of uncertainty, I made a new set of experiments on the composition of air, in the year 1824, with every precaution that I could think of to ensure accuracy. The air was collected in the middle of a green field, at some distance from all houses, and from marshes or ditches. And before examination it was carefully washed in caustic potash ley. The hydrogen which was used was prepared from a mixture of purified zinc and pure sulphuric acid diluted with distilled water. The retort in which the gas was extricated was quite filled with this dilute acid, and a portion of the hydrogen gas was allowed to escape before I began to collect it for use. The eudiometrical experiments were made while the hydrogen gas was coming over, and it was taken for every experiment directly from the beak of the retort, and, consequently, without standing any time

in contact with water. The air was kept in a bottle furnished with a ground stopper, and it stood inverted over water; the stopper being taken out only when a portion of the air was wanted for use.

The air was measured in a glass tube shut at one end, and capable of containing rather more than a cubic inch. This tube I had divided into hundredths of a cubic inch, by means of mercury, which I added by $\frac{1}{100}$th of a cubic inch at a time, and marked the height of the mercury after every addition, by a fine three-cornered file.*

I always took, for every experiment, one cubic inch or 100 volumes of air. The tube was quite clean, and after being filled, it was allowed to remain in a perpendicular position for three minutes, to give the water time to run down the sides. The air was then transferred to a Volta's eudiometer. The hydrogen gas was measured in the tube precisely in the same manner, and then transferred to the eudiometer and mixed with the air. The mixture was agitated by moving the eudiometer gently backwards and forwards for some minutes, in order to ensure the equal mixture of the two gases. The result of the experiments was as follows.

When 100 volumes of air were mixed with 40 volumes of hydrogen gas, and an electric spark passed through the the mixture, a detonation took place, and the diminution of bulk, determined by transferring the residue after combustion into the tube in which it had been originally measured, amounted to 57 measures. Now, the third part of this diminution amounts to the volume of oxygen gas which has disappeared,—water being a compound of one volume of oxygen and two volumes of hydrogen gases. But the third part of 57 is 19, which denotes the bulk of oxygen gas separated from 100 air. This makes the proportion of oxygen gas in air much less than Davy, Humboldt and Gay Lussac found it to be.

When the amount of hydrogen gas was made as high

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* The method of proceeding was this: A narrow slip of gummed paper was pasted longitudinally on the tube. After every addition of mercury a tangent line to the upper surface of the mercury was drawn with a pencil across this paper. And these lines were afterwards cut through, and a mark made on the tube by a file.

as 42 volumes, while that of the air continued 100, the result of the combustion was somewhat different. The diminution of bulk, in three successive experiments made exactly in the same way, was always 60 volumes. Now, the third part of 60 or 20 is the amount of the oxygen in 100 volumes of air, according to these trials. The azotic gas, of course, must be 80 volumes. Thus, the result of these experiments coincides with the opinion, that air is a chemical compound of four volumes azotic and one volume oxygen gas, or that it is a compound of two atoms azote and one atom oxygen.*

To verify these results, I placed a cubic inch or 100 measures of air in a glass vessel inverted over mercury, and then let up into the vessel a stick of phosphorus, of such a length as to reach to the top of the little vessel containing the air. The phosphorus and air were left together for two days, and till all action was at an end. The air was then transferred to the water trough, well washed to get rid of the phosphoric acid formed, and the residue measured in the usual manner. This analysis of air by phosphorus was repeated ten times. The following table shews the result.

No. of experiments.	Volumes of air.	Volumes of residue.
1	100	80·927
2	100	79·246
3	100	80·504
4	100	79·532
5	100	79·851
6	100	79·652
7	100	79·347
8	100	80·770
9	100	79·843
10	100	80·028
Mean	100	79·9335

The reader will observe, that in every one of these ten trials, the residue or the azotic gas in 100 volumes of air

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* If the hydrogen gas be more than 42 the diminution of bulk, after the explosion, increases very slowly; but 100 air and 48 or 50 hydrogen gave very nearly a diminution of 63, though never quite so much. This was the diminution found by Humboldt and Gay Lussac.

exceeded 79 volumes, the least residue being 79¼. In six of them, the residue was less than 80 volumes, while in four it exceeded 80 volumes. The mean of the whole, which must be very near the truth, gives 100 volumes of air composed of

<div align="center">

Oxygen gas . . . 20·0665
Azotic gas . . . . 79·9335
—————
100·0000

</div>

This only differs by $\frac{1}{302}$ part from 20 volumes of oxygen gas and 80 volumes of azotic gas. While the analysis of air, by means of hydrogen gas, when the hydrogen amounts to 42 volumes, and the air to 100 gives the composition exactly

<div align="center">

20 volumes oxygen gas.
80 volumes azotic gas.

</div>

These experiments, I conceive, leave no doubt whatever, that the real constitution of air, freed from all traces of carbonic acid, is 20 volumes oxygen and 80 volumes azotic gas.

But if this be the constitution of air, and if its specific gravity be unity, it is easy to deduce the specific gravity of oxygen gas and azotic gas. Let the atom of oxygen = 1, and that of azotic = 1·75.

Since air is a compound of 1 atom oxygen and 2 atoms azote, it consists in weight of

<div align="center">

Oxygen 1· or $22·22\dot{2} = a$
Azote . 3·5 or $77·77\dot{7} = b$
—————
100

</div>

Let x = specific gravity of oxygen gas, and
    y = specific gravity of azotic gas.

$$\frac{x + 4y}{5} = 1. \quad \text{Hence } x = 5 - 4y.$$

$$x : 4y :: a : b. \quad \text{Hence } x = \frac{4ay}{b}$$

$$5 - 4y = \frac{4ay}{b} \text{ and } y = \frac{5b}{4a + 4b} = 0·972\dot{2}$$

$$x = 5 - 4y = 5 - 3·888 = 1·111\dot{1}.$$

Thus, it appears that the specific gravity of pure dry oxygen gas is 1·111i, and that of azotic gas 0·9722.

Let us see how near the different experimenters who at-

tempted to determine the specific gravity of oxygen gas, have come to this result.

The first experimental determination of the specific gravity of oxygen gas, which has any pretension to accuracy, is that of Kirwan, related in his "*Essay on Phlogiston,*" p. 25. He procured the oxygen gas from the red oxide of mercury, and dried it by leaving it for 24 hours in contact with concentrated sulphuric acid. He obtained 1·103 for the specific gravity.

He informs us that the goodness of this oxygen gas was such, that, when one measure of it was mixed with two measures of common air over water, the gaseous residue unabsorbed amounted to $\frac{3}{10}$ths of a measure. If this trial was made (as it is probable it was) in a glass tube, whose diameter was under an inch, it will indicate that the oxygen, whose specific gravity was taken, contained the tenth part of its volume of azotic gas. Or, admitting that half the residue was nitrous gas, the oxygen would still contain 5 per cent. of azotic gas. Now, the specific gravity of such a mixture would be 1·1041, which is almost exactly that obtained by Mr. Kirwan.

About the year 1806, Biot and Arago determined apparently with great care the specific gravity of five different gases, and they state that of oxygen gas to be 1·10359, which coincides with the determination of Kirwan. But we have complete evidence that the determination of these distinguished philosophers cannot be perfectly accurate. For they give the specific gravity of azotic gas 0·96913. Now, if air consists of 21 volumes of oxygen and 79 volumes of azotic gas, according to the common determination, the specific gravity of common air deduced from that of the two gases, as determined by these gentlemen, would be not 1 as it ought to be, but 0·9973766; shewing clearly, that the specific gravity, either of the oxygen or azotic gas, or both of them, is somewhat below the truth. If air be a compound of 20 volumes of oxygen and 80 volumes of azotic gases, (as I have shewn from experiment), the specific gravity of air deduced from that of the oxygen and the azotic gas, as determined by these gentlemen, would be 0·9960220. It is clear, therefore, that the specific gravities, as determined by Biot and Arago, cannot be perfectly exact.

The next experimenter is M. T. de Saussure, who, in his " *Observations on the Combustion of different kinds of Charcoal, and on Hydrogen Gas,*" published in the year 1809. (*Annales de Chimie,* lxxi, 254,) gives a determination of the specific gravity of oxygen gas loaded with humidity at 54°½. According to his statement a cubic decilitre of oxygen gas, at the temperature of 54°½, and when the barometer stands at 29·834 inches, weighs 1·3552 French grains. It follows from this, that at the temperature of 60°, and under the same barometrical pressure 100 cubic inches of oxygen gas weigh 35·2018 grains troy. Now, if 100 cubic inches of air at 60°, and under the mean pressure of the atmosphere, weigh 31·0117 grains, as results from the experiments of Dr. Prout, the specific gravity of oxygen gas, as determined by De Saussure, is 1·13521. Notwithstanding the great care which De Saussure bestowed on this experiment, there can be no doubt, that his determination is too high. For, if air be composed of 21 volumes of oxygen and 79 volumes of azotic gas, and the specific gravity of common air 1, then the specific gravity of azotic gas deduced from Saussure's number for oxygen would be 0·96405, which is certainly below the truth. If air be a compound of 20 volumes oxygen and 80 azotic gas, the specific gravity of azotic gas would be only 0·96369 which is still lower.

In the year 1820, I devoted almost the whole of the summer to the determination of the specific gravity of gases. I got an apparatus constructed for the purpose, and was at uncommon pains, both in preparing the gases, and in obtaining them in a state of as great purity as possible. The oxygen gas was prepared from chlorate of potash, and contained no sensible admixture of azotic gas. My mode of proceeding was to weigh a flask filled with dry air; to exhaust the flask, and weigh it in that state. This gave the weight of air removed from the flask by the air pump. The flask was then filled with the oxygen, taking care to allow it to remain in contact with the oxygen in the gas holder, till it had acquired the temperature of the room. The flask was then weighed. The increase of weight gave the weight of a volume of oxygen gas equal to that of the air withdrawn from the flask by the air pump. I had only to divide the weight of the oxygen by that of the air, the quotient gave

the specific gravity of the oxygen gas. Three trials made in this way all agreed with each other, and gave the specific gravity of oxygen gas 1·1117.—(See *Annals of Philosophy*, xvi. 163.)

During the course of the same year 1820, an elaborate set of experiments on the specific gravity of oxygen, azote, hydrogen, and carbonic acid gas, was made by Berzelius and Dulong, with all that attention to minute accuracy which distinguishes these eminent chemists. (*Ann. de Chim. et de Phys.*, xv. 386.) The specific gravities of oxygen and azotic gases which they obtained are as follow:

Oxygen gas 1·1026
Azotic gas 0·976.

The specific gravity of oxygen gas as determined by these chemists coincides almost exactly with the previous determinations of Kirwan and of Biot and Arago.

But, notwithstanding the great care bestowed upon these experiments, it is demonstrable that they are not perfectly correct. For the specific gravity of air deduced from them (supposing air a compound of 21 oxygen and 79 azotic gas) would not be 1 as it ought to be, but 1·002586. And, if air be composed of 20 oxygen and 80 azote, its specific gravity deduced from the above numbers would be 1·00132.

Such are the most accurate experiments to determine the specific gravity of oxygen gas which have yet been made. None of them is perfectly correct. But as the errors in all probability lie on different sides, we may conclude, that by taking the mean of the whole, we shall obtain a number exceedingly near the truth. The following little table exhibits these results.

Specific gravity of oxygen gas according to

Kirwan, ...	1·103
Berzelius and Dulong, .	1·1026
Biot and Arago, . . .	1·103
De Saussure, ...	1·1352
Thomson, ...	1·1117
	5·5555

Mean, = 1·1111

Thus, we obtain for the mean of the whole, the very num-

ber deduced from the hypothesis, that air is a compound of 20 volumes oxygen and 80 volumes azotic gas; a hypothesis, the truth of which I think I have proved by experiment. I consider it then established upon the clearest evidence, that the specific gravity of oxygen gas is $1\frac{1}{9}$, and that air is a chemical compound of two atoms azote and one atom oxygen.

From the specific gravity of oxygen gas thus found, that of azotic gas is deducible with absolute certainty, and must be, as I have already shewn, 0·9722.

Having thus established the specific gravity of oxygen and azotic gases, let us apply this information to the determination of the atomic weights of some other substances.

1. The atomic weight of sulphur, according to Berzelius, is 2·01165. Dr. Turner, in his Elements of Chemistry, has adopted this number as he has several others of the atomic numbers given by Berzelius. And he has published in the " *Philosophical Transactions*" a set of experiments to shew that either my number for sulphur or potassium is wrong. And, when we inspect his book, we perceive by the numbers which he has adopted, that he considers both my numbers as erroneous.

It has been established by experiments, which I have often verified, that when sulphur is burnt in dry oxygen gas, the bulk of the gas is not altered, but it is converted into sulphurous acid gas. Let us, therefore, determine the specific gravity of sulphurous acid gas. It will enable us to establish the atomic weight of sulphur with much greater accuracy than by any other mode of experimenting. The specific gravity of this gas was determined by Kirwan to be 2·265 (*On Phlogiston*, p. 29.) Davy afterwards took its specific gravity, and found it 2·1930. Now, the mean of these two determinations is 2·229. So that, according to these old determinations, the oxygen gas, when converted into sulphurous acid, is only a very little more than doubled. In the summer of 1820, I made a very careful set of experiments on the specific gravity of this gas. It was prepared by boiling sulphurous acid on mercury, and contained no appreciable quantity of foreign matter, being totally absorbed by the peroxide of lead. I took the specific gravity three times in succession, and obtained the following results.

1st trial, 2·2221
2d trial, 2·2221
3d trial, 2·2223
_____
6·6665
_____

Mean = 2·22216

Now, this number may be considered as exactly double the specific gravity of oxygen gas. For it is only $\frac{1}{74000}$th part less, a difference far within the limits of the unavoidable errors in such delicate experiments, in which the amount of gas weighed scarcely exceeded 50 grains. The atomic weight of sulphur deduced from this specific gravity is 1·9999, or not $\frac{1}{20000}$th part less than 2. It certainly is not more than 2 as Berzelius concludes from experiments made, I admit, with great care; but certainly not susceptible of the same accuracy as we can attain in taking the specific gravity of a gas.

Thus, I conceive I have proved to the satisfaction of the most squeamish chemist, that the atomic weight of sulphur is 2. And that Berzelius's number, and, of course, Dr. Turner's is erroneous, exceeding the truth by rather more than a half per cent.

It is admitted on all hands, that the oxygen in sulphuric acid is to that in sulphurous acid as 3 to 2; that sulphurous acid is a compound of 1 atom sulphur and 2 atoms oxygen, and sulphuric acid of 1 atom sulphur and 3 oxygen. Hence, the weight of an atom of sulphurous acid is 4, and of sulphuric acid 5. Berzelius's numbers, and, of course, those of Dr. Turner, who has adopted them, are

Sulphurous acid, 4·01165
Sulphuric acid, 5·01165

The long string of decimals after the whole numbers 4 and 5 results from a small error in the atomic weight of sulphur which I have just pointed out.

2. Berzelius has pitched upon 0·76438 as the atomic weight of carbon; while Dr. Turner considers 0·765 as the true atomic weight relying on the recent analyses of vegetable substances. Now, let us see what light will be thrown on the subject by a knowledge of the specific gravity of carbonic acid gas, which is a compound of oxygen and carbon.

We may omit the results obtained by Cavendish and Lavoisier; because, though their experiments were made with great care, their methods were not susceptible of sufficient precision. Biot and Arago determined the specific gravity of this gas, in 1807, to be 1·5196. Berzelius and Dulong, in 1820, make its specific gravity 1·5245. Three trials of mine, made during the summer 1820, (which agreed with each other to the fourth decimal place), gave the following results.

1st Trial,	.	.	.	1·5266
2d Trial,	.	.	.	1·5268
3d Trial,	.	.	.	1·5268
Mean,	.	.	·	. 1·52673

My precautions in preparing the gas were such, that I have every reason to believe that it was very nearly pure. Now, when charcoal is burned in oxygen gas, the bulk of the gas is not altered; it is merely converted into carbonic acid. If we subtract the specific gravity of oxygen gas from that of carbonic acid gas, the remainder must be the weight of carbon united to a volume of oxygen gas.

Specific gravity of carbonic acid	1·5267
Specific gravity of oxygen gas	. 1·1111
	·4156

Hence, it appears that carbonic acid is a compound of

Oxygen	.	.	1·1111 or 2
Carbon	.	.	0·4156 or 0·748.

It is evident that the atomic weight of carbon, from my specific gravity of carbonic acid is ·748. If we were to take the specific gravity as determined by Berzelius and Dulong the atomic weight of carbon would be only 0·744. The reason why Berzelius makes it so high as 0·764 is, that he estimates the specific gravity of oxygen gas too low. Why I make the atom of carbon 0·75 instead of 0·748 will appear immediately.

3. The specific gravity of hydrogen gas was first determined with accuracy by me in 1820. For, when Biot and Arago made their experiments in 1807, the effect of moisture in altering the specific gravity of the very light gases

was not sufficiently understood, and, of course, was not guarded against. My results with pure dry hydrogen gas were as follow :

1st Trial . . .	0·06954
2nd Trial . . .	0·06933
3rd Trial . . .	0·06933
Mean	0·0694.*

Berzelius and Dulong made experiments on this subject during the same year, and they state the specific gravity of pure dry hydrogen gas, at 0·0687. But, from other considerations, they pitched upon 0·0688 as the true specific gravity.

Now, it is admitted on all hands, that water is a compound of one volume of oxygen gas, and two volumes of hydrogen gas. If we take my specific gravity, this gives us for the constituents of water by weight,

Oxygen, . . 1·1111　　or 1
Hydrogen, . 0·0694 + 2 or 0·1249.

This number is within less than $\frac{1}{1000}$th part of 0·125.

Berzelius and Dulong's specific gravity of hydrogen gas, gives us the composition of water,

Oxygen . . . 1·
Hydrogen . . 0·1238

But, Berzelius from other considerations, (thus tacitly giving up the accuracy of his specific gravities,) has adopted 0·124796 (or the half of that number, which comes to the same thing) as the weight of an atom of hydrogen, while Dr. Turner adopts my number, or 0·125, as representing the true atomic weight of that body.

Let us now see what conclusions can be formed from the experiments made, to determine the constituents of water by direct combination. By far the most accurate experiments on this subject, are those of Berzelius and Dulong. They passed a current of hydrogen gas through a glass tube filled with black oxide of copper, and heated by a spirit lamp. The hydrogen gas was dried before reaching the oxide of copper, by passing it through a tube filled with fragments of fused chloride of calcium, and the water formed was collected in another glass tube, also filled with

* Annals of Philosophy, xvi, 168.

fragments of chloride of calcium. The loss of weight sustained by the glass tube containing the oxide of copper, gave the quantity of oxygen employed, while the increase of weight in the chloride of calcium tube, gave the quantity of water formed. The result of the experiments was, that water is a compound of

>Oxygen . . 889 or 1
>Hydrogen . 111 or 0·12486

A number which does not differ by one-thousandth part from that deduced from the specific gravity of hydrogen gas, as determined by my experiments. The number 0·125 being generally adopted in this country, as the true atomic weight of hydrogen, I consider it as needless to enter into the subject more at large, otherwise it would be easy to shew, that the number adopted by Berzelius, differs from mine, by a quantity certainly within the limits of the unavoidable errors to which such experiments are liable.

4. Let us now see what the atomic weight of azote must be, determined from the specific gravity of oxygen and azotic gases. I have shown, that the true specific gravity of oxygen gas is 1·1111, and of azotic gas 0·9722. Now, 1·1111 : 0·9722 :: 16 : 14. So that the weight of equal volumes of the two gases are to each other as 16 to 14.

There are two gaseous compounds of oxygen and azotic gas; namely, *nitrous gas*, or *deutoxide of azote*, as it is now commonly called, which is a compound of 1 volume of oxygen and 1 volume of azotic gas; and *nitrous oxide* or *protoxide of azote*, which is a compound of 2 volumes of azote and one volume of oxygen gas. Nitrous gas is composed by weight of

>Oxygen 1·1111 or 16 or 1
>Azote   0·9722 or 14 or 0·875

Nitrous oxide of

>Oxygen 1·1111   or 16 or 1
>Azote   0·9722 × 2 or 28 or 1·75

Thus, there are two numbers, which may either represent the atom of azote, according as we consider nitrous gas or nitrous oxide as composed of a single atom of oxygen united to a single atom of azote. Mr. Dalton adopted the first of these numbers, in his original treatise on the atomic theory, in his Philosophy of chemistry, and his view of the subject

has been embraced by Berzelius, who makes the atom of azote to weigh 0·88518, a number which exceeds the true weight by almost one per cent. Dr. Wollaston on the other hand considered nitrous oxide as a compound of one atom oxygen and one atom azote. I embraced the same view and my number has been adopted by Dr. Turner. We make the atom of azote 1·75. The arguments in defence of these two systems are so equally balanced, that it is impossible to say which are right or which wrong. But, perhaps, before I finish this paper, I may be able to suggest some considerations which will throw the balance on one side and make the other kick the beam.

From the evidence adduced in the preceding part of this paper, I consider myself entitled to conclude, that the atomic weights of oxygen, sulphur, carbon, hydrogen and azote are as follow :

$$\begin{array}{ll} \text{Oxygen .} & 1 \\ \text{Sulphur} & 2 \\ \text{Carbon .} & 0·75 \quad \text{or} \quad ·375 \text{ or } 1·5 \\ \text{Hydrogen} & 0·125 \text{ or } 0·0625 \\ \text{Azote .} & 1·75 \quad \text{or } 0·875 \end{array}$$

The experimental result for hydrogen is 0·1249, which deviating less than $\frac{1}{1200}$th part from 0·125, I assume that number for the true one. It is generally adopted in Great Britain, and also by many of the Continental chemists. And it has the important advantage, that it makes the atomic weights of the other simple substances, simple multiples of that of hydrogen.

The atom of sulphur deduced from experiment being 1·9999, I presume the most squeamish will not hesitate to admit that the true number is 2.

The atom of carbon from experiment is 0·748. Now, as Berzelius' number is 0·764, and as $0·75 = 0·125 \times 6$, we cannot hesitate to adopt 0·75, or its half, or double, as the true number.

The atom of azote deduced from the composition of air is 1·75 or 0·875.

We shall next consider how far these numbers are supported by the specific gravity of the gaseous bodies into which these bodies enter as constituents.

(To be continued.)

ARTICLE III.

Experiments and Observations on Visible Vibration.
By CHARLES TOMLINSON, Esq.

(*Continued from* vol. ii. p. 133.)

76. In three papers already published in the *Records of General Science* during the last year, I detailed, as far as my observations had then extended, several phenomena arising from the vibration of mercury and other fluids contained in glass vessels; and, in continuing the investigation, I have again availed myself of the co-operation and assistance of my friend Mr. Dodd, whose correct musical ear and love of science have rendered the inquiry delightful to both of us.

It will be recollected, that the surface of the mercury, when under the influence of vibration, was described as being rippled by undulæ, which assumed such an arrangement as to produce a highly pleasing figure or series of figures.(8)

77. Since the publication of the former papers, I have frequently recurred to this particular part of the subject, with the view of ascertaining whether the figures produced were, in all cases, similar to each other, or whether they were changed or modified by the size of the containing vessel, the bulk of mercury, or the extent of mercurial surface in relation to the bulk; and, although we have not, as yet, succeeded in eliminating any general law on the subject, we have ascertained, that various figures can be procured from the mercurial surface which are modified, and, in some cases, totally changed, according as one or more of the conditions, just stated, are observed; and, above all, according to the number of vibrations per second to which the mercury is subjected. We have, therefore, thought the subject, even in its present incomplete state, of sufficient interest to allow us to convey an idea of the nature of these figures, as it is obvious, that in proportion as we become acquainted, by the disturbance at the surface of the mercury, with the impulses which the fluid receives from the glass, so shall we obtain clearer ideas of the reciprocal action of the fluid and the glass when vibrating, and of the nodal divisions of the latter.

78. A large number of figures has been produced by employing one glass as a standard, a soda-water glass, for instance, yielding a certain note, which is reduced, at musical intervals, by adding successive quantities of perfectly clean and pure mercury, and the figure is produced by passing the moist finger round the edge of the glass. The figure is then carefully examined and copied; each figure answering to a certain note or half-note.

79. Results having been thus obtained, the question naturally suggested itself, whether a certain sound is necessarily accompanied by a certain figure? It seemed obvious, that when two unisonant glasses were employed together, with the same quantity of mercury in each, that the same figure would be obtained from each. Thus, where two glasses were employed, the fundamental note of each being D sharp, and four fluid ounces of mercury are required by each to lower their respective fundamental notes to C sharp, the resulting figures, both in theory and in practice, are the same. So, also, with a glass, whose fundamental note is C sharp, if mercury be added below the axis(53), the same figure is produced as from the two former glasses; but we approach nearer the main question when two dissonant glasses are employed requiring dissimilar bulks of of mercury to accord them. Thus, one set of figures was produced from the surface of mercury contained in a glass, the fundamental note of which, when empty, was D sharp. In order to verify the first set of figures, another glass was taken similar in size and shape to the former, but whose fundamental note was C, differing from the former by three semi-tones. Both glasses were soda-water glasses forming part of my set of musical glasses(47), and they were chosen on account of their affording an extensive mercurial surface, and offering facilities for easy vibration by almost a touch. The bulks of mercury, of course, differed greatly in order to accord the two glasses; and yet, in many cases, the same figures were obtained from the second glass as had previously been obtained from the first. Thus, D, C sharp, C, B, B flat, G, F sharp and F, middle octave of flute, gave, nearly, and in some cases, exactly, the same figures for each note as in the first set, and, in all cases the dis-

tinguishing characters and peculiarities of the figures were
preserved.   One remarkable coincidence was observed in
the case of B flat, which in the first set yielded three varieties
of figure, and which in the second yielded one figure com-
posed of the peculiarities of all the three varieties in the
first set.   In some cases where four fans [fig. 3] enter into
the composition of the figure, the fans seemed to revolve,
while the barley-corn shaped figures and reflected edge
were stationary, thus, giving figures composed of fixed out-
lines and moving super-positions.

80. It will be understood, that, at present, we by no means
pretend to support the proposition, that every constant
musical sound is the generator of a constant figure.   Our
results, at present, can be taken scarcely even as approxi-
mations to the elucidation of the question.   The present
paper will shew something of our progress, as also the
doubt and difficulty in which this part of the inquiry is at
present involved. We hope, however, at some future period,
to reduce these figures to a system as perfect, and certainly
more beautiful in its results, as that which regulates the
figures of Chladni.

81. It has been before stated (20), that the reflected edge
of the glass may be seen in the mercury during vibration,
thrown into a series of nodes and ventral segments.   Such
a case is represented in

Fig. 1.
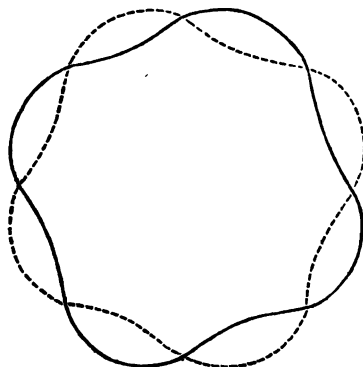
which seems to present the fundamental figure of all that
have been obtained, the reflected edge entering more or less
into the composition of the figures.

· 82. When a soda-water glass was partially filled with mercury until it produced, by friction with the moist finger round the edge, the note E flat; on looking down upon the surface the figure 2 presented itself.

Fig. 2.

83. The surface is broken up into concentric bands of undulæ, while there is at the same time a star of four points revolving in the direction, and with the velocity of the finger. The production of this star seems dependent on the number of nodes in the glass, and, when once produced, is projected in a circular course, by the vibratory changes, in the form of the glass.

84. The manner in which the undulæ seem to embrace each separate radius of the star, conveys an idea of the interference of four series or systems of undulations, proceeding severally from the centres of the four ventral segments towards the centre of the mercurial surface, and then acquiring a circular motion, subsequent to their propulsion from the glass, towards the centre.

85. It is difficult to form a correct judgment by engraved figures; but it will assist the conception to state, that the divisions between the light and dark portions of each radius are occasioned by a change in the direction of the undulæ, so that the phase or inclined side of each little hillock of mercury, presented towards the light, being different to those presented by the adjoining hillocks, the reflexion reaches the eye under different circumstances, and presents the appearance of a brilliant twinkling reflexion on the one hand, and of comparative darkness on the other. In

one portion of each radius, the compensating interference of the two contiguous systems of undulations is so exactly balanced, that the surface remains smooth and unruffled, and greatly adds to the symmetrical effect of the whole.

86. It is a necessary consequence of the last observation, that if the eye deviate from the perpendicular view of the mercurial surface, the form of the figure will vary according to the angle of deviation, and, thus present such diversity of form, that, were it not for the absence of colour, we might almost draw an analogy between the mercurial figures and those of the ever-varying kaleidoscope.

87. The convexity of the mercurial surface has considerable influence upon the figure, and as a small surface of mercury is comparatively more convex than a larger surface, a smaller or a larger glass presents an interesting change in the figure.

88. Thus, in a goblet smaller than the one previously used (82), sufficient mercury was poured in to produce the note G, when a figure resembling the figure 3 was presented.

Fig. 3.
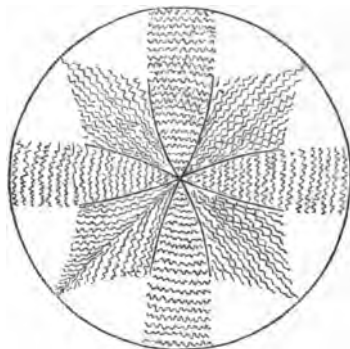
In this case as in the former the outlines of the figures are formed by a change in the direction of the undulæ, and receives many pleasing modifications by varying the angle of observation.

89. It appears, as indeed it is reasonable to suppose, that the alteration produced in the figure, by varying the size of the vessel, is also produced, in nearly the same degree, by employing various depths of mercury in an inverted conical or dome-shaped vessel, the proportion between the chord

and the radius of convexity of the surface varying with the quantity of the fluid.

90. By proceeding in this way many varieties of figures are produced, of which, perhaps, one or two more will suffice for our present purpose.

Fig. 4.   Fig. 5.
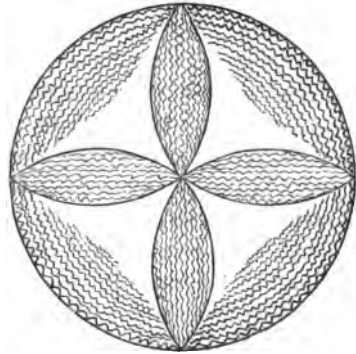
The same remarks which were made respecting the formation of the former figures will apply to these likewise.

91. To produce symmetrical figures, an essential point is to keep the mercurial surface clean and free from impurities, as the slightest particle of dirt will, in some measure, check the continuous formation of the undulatory curves.

92. But even this defect is the source of many new curves interlacing among, what we may term, the primary curves, and producing modifications in the figures, which, though not symmetrical, are still pleasing. Thus, when a small shot, or a particle of dirt is on the surface of the mercury, it becomes, when vibration has commenced, a focus of a system of concentric ellipses, the other focus of which is the centre of the mercury. These elliptic curves spread out from the prolate axis to a considerable distance, and produce very complicated curves by interference with the revolving star and the undulæ.

93. There will be as many of these systems of ellipses as there are impurities (if of an appreciable size), and, as the size of these ellipses depends on the distance of the particle from the centre of the mercury, they cannot join in a harmonious whole; but still, as the prolate axis of these

ellipses are in direct radial lines from the centre of the mercury, they together produce a star-like effect, which is by no means devoid of beauty. A figure may serve to assist the reader.

Fig. 6.
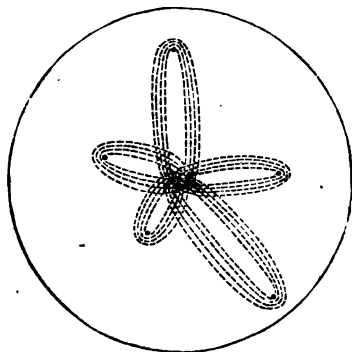
Here the revolving star is omitted for the sake of perspicuity.

94. When a needle is placed on the surface of the mercury (39), or a piece of iron wire (41), or any other substance (43), the motion of these substances is in a contrary direction to the finger, and contrary to the apparent motion of the star (39). This part of the inquiry will form the subject of a separate paper; but it may now be stated, that when a needle or wire is employed, a series of concentric ellipses is formed, of which, the needle or wire is the prolate axis, and other substances, depending upon their nature, form either foci or axes of ellipses.

A further inquiry into the nodal divisions of a glass goblet will also form the subject of a future paper.

Salisbury, January, 1836.

ARTICLE IV.

Observations on Zoophytes and Plants confounded with them.
By H. F. LINK, Professor of Botany, at Berlin.*

MUCH difference of opinion has existed with regard to the animal nature of Polypi. Some have regarded the common

* Ann. des Sciences Naturelles, ii. 321, 2d Ser.

stalk which supports the species of this class as the result
of an inorganic calcareous secretion, analogous to the shells
of molluscous animals. Cavolini and Schweigger, again, re-
gard it as an organized vascular axis having become rapidly
encrusted with calcareous matter. The observations of Link
upon *Plumularia falcata* and *Sertularia Cupressina* have
caused him to adopt the latter opinion. For, with a strong
magnifying power he saw coloured vessels distributed in
the trunk and branches of these Polypi. He considers that
these stalks increase by concentric layers, and that the cal-
careous matter is deposited in true cells. This calcareous
deposit was considered as characteristic of Zoophytes, and
was probably the reason why many Algæ were ranked in
this class. We know, however, that species of *Chara* are
supplied with a calcareous coating, and no one would dream
of separating them from the vegetable kingdom. With
the corals the only difference is, that this deposition takes
place so rapidly and abundantly, that we rarely see these
animals in their gelatinous state. M. Schübler has lately
observed a calcareous deposit, in regular grains, upon an
Alga, which he has therefore termed *Hydrurus Crystallo-
phorus*. But this deposit is not of a crystalline nature, as
it possesses neither the lustre nor transparency of true cry-
stals. Schweigger has accurately discriminated the Algæ
which were confounded with Zoophytes, having recognized
upon several of them crystalline grains, which from size,
form, &c., may be considered as little seeds *(seminules)*, and
determine the position of these beings in the vegetable
kingdom. Link chooses, for the subject of his memoir, the
anomalous *Algæ*, as the other plants have not been examined
with a sufficiently large magnifying power.

1. The first family is the *Halimedeæ*. When these bodies
are separated from their calcareous envelope, they present
a lamellar or membranous structure. The calcareous coat
is tender like chalk. It not only covers the external surface
of the animal, but sometimes also the internal surface. The
genus has been named, by Lamarck, *Flabellaria*, who has
placed it, with the gelatinous *Polypi*, among the *Spongia*,
to which it cannot belong. This genus is composed of two
others. 1st, *Udotea* of Lamouroux, comprehending *Flabel-
laria pavonia* (Lam.), which is, according to Link, the *Zo-*

naria pavonia, of which he described the fruit (*Horæ Berolinenses 7*). Its position is near the *Corallineæ*. 2d, *Halimedea* is articulated with compressed joints, interiorly calcareous, containing a fibrous marrow which unites the joints. The *H. Opuntia* (*Corallina opuntia*, Linn. *Flabellaria opuntia*, Lam.) has been carefully examined by Schweigger. He observed the fibres to be succulent filaments, which increase and ramify irregularly. The structure of the cellular tissue is decided. He, therefore, concludes, that the *C. opuntia* ought to be restored to the vegetable kingdom. Its parenchyma is formed of pentagonal or hexagonal cells as in plants, a structure which never occurs in animals. Link has also examined this species, and agrees with Schweigger. He finds, in addition, that the fibrous tissue, which forms the middle layer of the articulations, is composed of ramified leaflets as in an *ulva*. These leaflets form a membrane which receives the vesicular cells. These cells rarely angular are not in contact with each other. They do not constitute the membrane which contains them, as we see in the superior kinds of plants. Thus the structure of the *H. Opuntia* agrees with that of plants possessing a complicated organization. But it approaches that of the *Algæ*, for the *Halimedeæ* may be considered as compound *ulvæ*, and the *Fuci* may be viewed as compound *Confervæ*. The calcareous deposit is formed in cells within the interior of the plant, upon the two sides of the most internal fibrous layer.

Lamarck re-united the *Dichotomaria* with the *Sertulariæ*. Lamouroux divided them into two genera, viz. *Galaxaura* and *Liagora*. The first genus contains the true *Dichotomaria*, *D. fragilis*. The plants of this genus are much ramified; the articulations are rounded when first compressed and traversed by irregular membranes when they are dry. Both internal and external surfaces are covered with calcareous matter, which does not exist at the first period of their existence.

When viewed carefully, small holes may be observed irregularly spread, which, perhaps, permit the escape of the seed as in *Fuci*. When the calcareous deposit is removed by muriatic acid, by means of a magnifier, we see distinctly that the whole mass of the vegetable consists of interlaced

lamellæ as in the *Halimedeæ*. Over these plates are large
vesicular cells. Schweigger terms these cells filaments,
because, probably, he did not employ a sufficiently strong
magnifying power. When the calcareous matter is not
completely removed, the cells are observed to be filled.
The genus *Liagora* is distinguised from that described, by
the absence of articulations. The trunk of the plants of
this genus is ramified and covered with lime. The *Liagora
complanata* (*Agardh*), or the *Fucus lichenoides* (Esp.) is the
only species of this genus known to Link. It is compressed,
much ramified, with acute branches, green upon one side,
and calcareous on the other. When the plant is digested
for some days in muriatic acid, all the substance presents
the appearance of being divided into two large cells. By
the microscope, we observe, that these cells are united
by a membrane. If only a portion of the lime is removed,
and the plant be examined, we find a membrane of which
the side is covered with vesicles, and the rest of the
lime is dispersed in small portions over the surface of
this membrane. Agardh united the *Fucus distentus* with
this kind of *Liagora*, although it certainly does not belong
to it.

The cells of the *Fuci*, which differ much from the vesicu-
lar cells of which we have been speaking, resemble those
of the superior orders of plants.

The vesicular structure united in most of the *Halimedeæ*
with the lamellar ramifications, constitutes, in general, the
essential organization of those *Algæ* which approach them
in their external and internal structure.

The *Acetabulum Mediterraneum*, or *A Marinum* (Schweig-
ger), or rather, the *Acetabularia* of Lamouroux is a very
singular body which resembles the *Agaricus*, or stalked
Helotium. It consists of a round top, with a pedicle. It is
covered with lime, which may be removed by an acid, when
we wish to study its structure. The top is formed of tubes
which are first straight in the centre, and increase towards
the circumference. In each tube there is a canal which
often appears as if displaced; it is filled with a green and
granular matter. A similar tube is observed in *Conferva*
filled with the same substance as in the *Spyrogyri*. We
might say, therefore, that this body is a *Conferva*, if we

did not observe filaments arranged round the centre of the top, and supplied with distinct apertures.

Schweigger speaks of these filaments, and combats the opinion of Cavolini, who considers them as the filaments of parasitical *Confervae*. They appear to be similar to those which proceed from the mass of grains of the fuci, especially, *Fucus vesiculosus*. Hence, we may unite *Acetabularia* with *Halimedea*, or, form a peculiar family in which the *Polyphysa* (Lam.) may be placed.

The *Alcyonium Bursa*, Lam. *Fucus Bursa*, Turn. *Spongodium Bursa*, Schw. has been long considered an *Alga*. The same may be said of the *Alcyonium vermiculare*, Gmel. or *Vermicularia retusa*, Imperati, *Fucus tomentosus*, Turn. *Spongodium dichotomum*, Stackhouse. Stackhouse places this species in the genus *Codium* with the preceding, under the name *Codium tomentosum*, and Agardh has followed him. We have only to examine a *fucus* to be convinced, that it belongs to this family. All the Fuci consist of simple tubes containing a canal filled with a granular coloured mass. These are irregular when the plant is dry, and even affect different shapes when in life. The *fuci*, therefore, may be regarded as consisting of a number of filaments analogous to the *confervae*. The *Codium* is distinguished from common *fuci*, such as *F vesiculosus*, 1st, By very short and wide canals or cells. 2d, Because these cells appear on the surface of the plant. But these differences are too insignificant to form a proper separation of *Codium* and *Fucus*.

2. The second family is that of the *Corallineae*.

When we place the *Corallina vulgaris* in dilute muriatic acid we obtain the same form; but possessing a gelatinous consistence, with distinct articulations. When magnified, we observe transverse striæ of a reddish colour, and a great number of granules, as well as tubes, which are either empty or full of granules. These granules separate by slight pressure. All this substance consists of short straight cells, placed in a gelatinous matter, and in contact. The same structure is observed in *Corallina rubens*, which only differs from *C. officinalis* in colour. In the *C. rosarium* the structure is a little different. The calcareous matter is greenish. The granules are formed of striæ, which are connected with

the joints. Schweigger states, that he found parallel fila-
ments in the *C rubens;* but this appears to have arisen from
his not removing the calcareous matter completely. The
little seeds or grains approach the Coralline *zonaria;* but
the external form is very different; the frond of the latter
being without distinct articulations, and destitute of cal-
careous deposit. The parts which contain the seeds in
Zonaria are placed in concentric zones, as in the corallines;
they are ranged in concentric lines. The body consists of
more distinct cells than in Corallines.

The *Zonaria squamaria* undoubtedly belongs to the genus
Zonaria. Its consistence is greater and more like that of
the *Fuci.* Schweigger has remarked, that at a certain age
the *Z squamaria* is covered with calcareous matter, and is
changed into *Millepora coriacea,* Linn.

3. The *Zonariæ* form the third family, and Zonaria is the
only genus belonging to it.

4. The fourth family is that of the *Spongodiae.* Ten or
twelve years have elapsed since Link observed the distinct
sporules of the *Spongia lacustris,* (Spongilla lacustris Lam
Ephydatea Lamx), and annually has collected them near the
town of Spandau. They are about the size of millet seed, very
visible to the naked eye. They are distinctly not parasitical,
and are of a yellowish colour. When magnified, they ap-
pear as seed immersed in a soft matter, which rests on a
membrane consisting of fine tubes. The *Spongia officinalis*
possesses a similar structure, as well as the *S lacunulosa,
virgultosa* and *dichotoma.*

Ehrenberg has observed sporules in several sponges from
the Red Sea.

Dr. Grant detected a motion in the water which pro-
ceeded from the apertures placed at the surface, without any
accompanying contraction. At the same time membranous
matter was discharged, which he took to be the excrement
of the animal. This appeared, however, only accidental,
and Link is inclined to consider the motion of the water as
similar to that in the *Chara.* In the *Spongia panicea* Dr.
Grant observed eggs which possessed a peculiar motion,
like those of the *Gorgons.* Link says that the *Alcyonium
paniceum* is found on the coast of England, but not the
S panicea, which is quite unknown to him. But even if

motion were detected in the sponges, he considers that this would not be sufficient to establish their animal nature, for the same has been noticed in *confervae.* He thinks that the absence of polypi, the existence of distinct sporanges in the *Spongilla,* and the analogy between the latter and true sponges are sufficient to separate the sponges from the Zoophytes and to class them with the Algae.

It is true that the structure of sponges is different from that of other *algae,* but the structure of the latter presents striking modifications. Grant has observed in sponges very fine points formed of pure silica, which correspond with the fine fibrous points which we observe in the spongilla, and which possess equal solidity and tenuity. In other respects these plants approach near the first animals. They are composed of a thick tissue, consisting of filaments and tubes as in sponges, which is less reticulated, and is not covered by a gelatinous membrane. The tubes are covered with spiral points which are the commencement of the branches. In some cases we remark elongated branches. They are perfectly transparent, tenacious, and dissolve with effervescence in dilute muriatic acid, leaving only a slight portion of membrane.

They are, therefore, covered with carbonate of lime completely transparent, which is rarely met with in the Zoophytes.

But the animal nature of these beings is proved by the large cavities, which occupy not only the interior of the youngest branches in the *Alcyonium arboreum,* but which are prolonged through the crust to the surface, where they terminate in polypi.

Hence, we see how slight is the distinction between plants and animals.

The animal matter is so to speak opposed to vegetable substance. The first disappears in the sponges, and vegetable matter remains.

Olivi and Bertolini consider the *Nullipores* as calcareous deposits. Schweigger takes them for Zoophytes, which are transformed immediately after their production into calcareous matter. He cites, as a proof of this opinion, that after having placed them in muriatic acid, a gelatinous body still remained of the figure of the *Nullipores.*

Link never could observe this. All these species, according to him, possess cavities which proceed far into the branches, and possess a structure very analogous to *Osteocolla* or calcareous tuff, which covers *junci* and other plants in ponds. In his opinion, the *Nullipores* are nothing else than a similar calcareous deposit, which are formed round marine plants.

ARTICLE IV.

On a Vegeto-Calcareous Hydrate, produced from Byssus Floccosa. By ROBERT D. THOMSON, M. D.

WHILE lately viewing the cellars of Henry Gill and Co., in Mark Lane, my attention was attracted by observing a number of gelatinous looking stalactites hanging from the roof. The intervals between their bases were occupied by large tufts of *Byssus floccosa* and *Racodium cellare*, which in some instances appeared to be intermixed with them, and to have assumed that black and soft state, so remarkable in these curious plants; a form in which they have been recommended as styptics. The *Byssus floccosa* is a very common inhabitant of cellars, and is characterized, when in the state of maturity, by its beautiful snow-white tufts, arranged in an orbicular form; the filaments or elements of the tuft being close, simple, and parallel. When this plant is situated on a roof, timber, or cask, it assumes a pendent form, and, at first sight, has a close similarity to cotton. When pressed between sheets of blotting paper, the filaments are closely compressed and stick to the paper; the plant then assuming the appearance of one of the fleshy fungi after being subjected to pressure. The filaments cross each other, and from their glutinous nature are very liable to adhere to each other, and to any object with which they may happen to come in contact. When examined by the microscope, they appear to be fistulose and jointed, or at least, the internal vacancy is not cylindrical, but contracted and dilated at irregular and distant intervals.

I have noticed the *Racodium cellare* as occurring intermixed with the *Byssus* because I observed several filaments which answered the description given by the best botanical

authorities, and not because I believe it to be distinct from the *Byssus;* for a very trivial examination was quite sufficient to convince me that both plants are mere varieties, or different states of the same species.　The only distinction seems to be the darker colour of the filaments, and their more rigid consistence, indicating a more advanced state than the softness of the *Byssus.*　Whether this distinction be accurate or not, however, it seems to have no influence upon their subsequent state, as the filaments of both plants gradually become more moist, collapse, and run together, giving rise to the gelatinous stalactites to which I have already referred.　These possess a yellowish brown colour, and a taste like gum Arabic; their length is from two to three inches, and their diameter from $\frac{1}{4}$ to $\frac{1}{2}$ inch, tapering gradually from a broader base, which measures about $1\frac{1}{2}$ inch across.　Some specimens were about 6 inches in length. The substance of which they consisted, exhibited neither an acid nor alkaline re-action, when test paper was exposed to its influence.

　A small portion, when heated in a tube over a spirit lamp, disengaged a great quantity of vapour, and a vegetable odour similar to that produced by the destructive distillation of the woody matter of herbaceous plants. The vapour was condensed, and collected on a watch glass; it exhibited neither an acid nor alkaline re-action, possessed no taste, and left no residue, when evaporated to dryness.　It was, therefore, pure water.　A substance resembling dry membrane or glue when spread out thinly over a surface and dried, remained after the separation of all the water.　When the heat was urged further, smoke began to rise, and continued to augment until nothing remained but a white powder mixed with some carbonaceous matter; the former dissolved with effervescence completely in pure dilute nitric acid; the solution afforded a white precipitate with oxalate of ammonia, and a few flocks fell when caustic ammonia was poured into the solution, indicating the presence of carbonate and phosphate of lime.

　A large portion of the gelatinous mass was boiled for some hours in a flask with a considerable quantity of water; the solid matter was allowed to settle, and the supernatant liquid was withdrawn by a sucker.　The liquid thus sepa-

rated, when evaporated to dryness, left a minute portion of yellowish membranous matter, exactly similar to the residue remaining after the evaporation of the water from the gelatinous mass itself.

When the mass is digested in *alcohol*, the water is taken up, and the vegetable matter remains.

Muriatic acid, with the assistance of heat, produces effervescence, and separates the mass into a few flocks, which float in the liquid.

Caustic Potash dissolved a portion of the mass; but the greater part remained swimming in the ley in the form of aluminous like flocks, which were phosphate of lime.

Fifty grains of the mass were carefully evaporated on the sand bath in a platinum crucible, till the whole of the water was expelled, and a yellowish membranous substance lined the internal surface of the crucible. The loss amounted to 48·77 grains, leaving for residue 1·23. The residuum was exposed to a red heat. The white powder which remained weighed ·125 grain. It dissolved with effervescence in dilute muriatic acid, and was precipitated by caustic ammonia and oxalate of ammonia. The membranous matter, when digested in water, swelled up like gum, and possessed an appearance similar to the original gelatinous mass; its taste was also the same.

The composition of the gelatinous hydrate was therefore

Water, ...	97·53
Vegetable matter, ...	2·21
Carbonate of lime and Phosphate of lime,	·25

100·00

ARTICLE V.

The Art of Dyeing. *

Of the Water.—The water which the dyer employs must be as pure as possible. The purest and clearest colours are obtained in all cases by the employment of distilled water,

* From the " *Farben Chemie.* 1 *Thiel ; Die Kunst zu farben gegrundet auf das Chemische Verhalten der Baumwollenfaser zu der Salzen und Sauren.*" Von Dr. F. F. Runge.—*Berlin,* 1834.

as it is free from foreign matter. Next to it comes rain and river water; but spring water does not answer for dyeing, because it contains a quantity of lime, which falls down with the colouring matter in the form of an insoluble precipitate, and thus occasions great loss of colouring matter. The same happens when well water is employed for boiling dye-woods and roots. When the reddish yellow-coloured Dutch madder is boiled with pure water, the residue, after drying, acquires a light brown colour, and imparts to a solution of alum a faint red colour when both are boiled together. When, on the other hand, spring water is substituted, the residue is dark reddish brown, and the solution of alum, by being boiled with it, becomes dark red. In the first case, the quantity of madder-red remaining in the residue is much less than in the second. The madder-red has been precipitated by the lime of the spring water, and has imparted to the residue the dark colour, and is dissolved by the alum solution. Pure water, therefore, dissolves more madder-red than water containing lime does.

With Fernambuc and logwood the same results are obtained.

To determine whether river water is fitted for dyeing, the above experiment may be had recourse to, or a specimen may be dyed, first with the water to be tested, and then with pure distilled water.

The presence of lime may also be detected in such waters by chemical means. When soap is added to water containing lime, and the mixture heated, it becomes turbid. Carbonate of soda makes it milky, and throws down a white precipitate. Oxalate of potash acts in a similar manner.*

Water which flows over marshy ground often contains in solution a quantity of decomposed vegetable matter, which especially acts upon oiled cotton and soils it. Such water is likewise injurious to the white ground. Chrome colours especially chrome-yellow lose their lustre and become ugly. This proceeds principally from the sulphur contained in

* Oxalate of ammonia answers better than this salt, because the precipitate can be readily freed from alkali by heat; while, if oxalate of potash is employed, the precipitate requires to be thoroughly washed, (and this is difficult to effect), before ignition.—EDIT.

the water, which combines with the lead of the chrome-
yellow, and makes it black.

The water in dyeing plays a very peculiar part, as it must
first dissolve the colouring matter before the latter can be
taken up by the mordanted cotton; so there exists an oppo-
sition between the water which holds the colouring matter
in solution, and between the cotton which will cause the
water to abandon the colouring matter, and render it in-
capable of dissolving it. Much water, therefore, increases
the influence of this opposing property in the mordanted
cotton; a small quantity of water, on the other hand,
diminishes it. When we wish, therefore, that a solution
shall exhaust as much colouring matter as possible, a small
quantity of water must be employed. With different dyes,
and different mordants the result varies. Thus, with 1 loth
(·469 Troy ounce) logwood-blue and 10,000 loths (4690 oz.)
of water, cotton impregnated with the acetate of alumina
mordant completely exhausts the colour at once; while a
solution of berry-yellow, containing the same quantity of
water can be deprived of the whole of its yellow colour,
only by repeated boiling with fresh cotton.

Clay is a substance, which, in this respect, assists the
water. Its striking action with madder colours shews this
in a very distinct manner.

Substances which form a thick slime with water as tra-
gacanth and salap cannot be diluted anew with propriety
with much water, as the mixture will not be equable. They
must first merely be moistened with water, that they may
swell up; then the necessary quantity of water should be
gradually added. The same proceeding is necessary with
isinglass.

Alumina, which is used for reserves, must, in the same
way, first be moistened with water, and then diluted, other-
wise it loses its combining power.

Sulphuric acid.—This acid is also termed Oil of Vitriol,
although this name is applied with more propriety to the
sulphuric acid which fumes or gives off a white smoke
when exposed to the atmosphere. Such acid is to be pre-
ferred for dissolving indigo.

If a drop of sulphuric acid falls upon a piece of cotton,
the spot becomes black, and a hole is formed; consequently,
sulphuric acid corrodes cotton.

If we mix 4 loths (1·876 oz.) water, with 1 loth (·469 oz.) sulphuric acid, (which must be done by pouring gradually the sulphuric acid into the water, not by pouring the water at once into the sulphuric acid, as with great quantities a heat is produced); and, after cooling, place a portion of cotton in the mixture ; the cotton has not lost its firmness in the course of an hour. But, if the saturated cotton is hung up in a warm place, as soon as it begins to dry, it becomes tender and at last falls to pieces like tinder. This also happens when the acid is even more dilute. Hence, it follows, that no dilute sulphuric acid should be allowed to come in contact with cotton, when the latter can become dry, or the water evaporate. Boiling with dilute sulphuric acid is also deleterious.

On the other hand, we may allow, without any risk, cotton to lie for 12 or 24 hours at a temperature of 65°¼ to 86°, in a mixture of 100 lbs. water, and 1 to 2 lbs. sulphuric acid, as is done in clearing the cotton for printing ; when it is well washed with pure water.

To acidify mordants, sulphuric acid is employed. It is in these cases, however, rendered less deteriorating by mixture, but can still be rendered injurious by exposure to a stronger drying temperature. Hence, it is obvious, that such cotton cannot be exposed to strong heat. If sulphuric acid be added to a thick and hot paste of starch, the latter becomes fluid like water ; but if the paste is previously allowed to cool, and then dilute sulphuric acid is added, no such effect takes place. Mordants which are thickened with starch should therefore be allowed to cool, lest they should be injured by sulphuric acid. By a contrary process the same will be destroyed. Sulphuric acid deprives cotton of the mordant partly by itself, partly in combination with tartar. It may thus, therefore, be employed to renovate injured pieces. It further destroys a quantity of coloured compounds, which is likewise necessary in order to re-dye pieces which are injured in dyeing.

Both will be farther noticed in the chapter *on the renovation of injured goods.*

Muriatic acid.—This acid which is employed in chemical manufactures has commonly a yellow colour and a penetrating odour. A colourless is to be preferred to a coloured

acid, although the latter in most cases can be employed. Muriatic acid commonly contains some sulphuric acid. This is injurious in many cases, especially in mixtures of mordants, in which salts of tin and muriatic acid occur at the same time; such is the case in the *Tin Mordant*, No. 1. We dissolve 1 lb. tin salt in 1 lb. water, and add 1 lb. muriatic acid. If any sulphuric acid be present, the liquid asumes a brown colour, and acquires an odour of sulphuretted hydrogen, which does not take place when the muriatic acid is free from sulphuric acid.

In order to render this tin mordant fit for use, it should be placed in a close flask at rest; it will then become clear by the deposition of a brown sediment. The disagreeable odour is still retained, but the dye is not injured.

Muriatic acid corrodes cotton. This takes place more rapidly according as the heat is stronger. It is necessary to employ with precaution the mordants which contain free muriatic acid; and, pieces which are printed with it, must not be allowed to hang for any length of time.

But, when the mordants, displaced by muriatic acid, are by preference caustic, which iron, lead, and other colours require, and muriatic acid is the menstruum which dissolves these colours; it loses thereby much of its injurious property, because it rather acts upon these dyes than upon the cotton threads. Yet, a piece printed with such caustic mordants must not be allowed to hang up for any length of time; but it is indispensably necessary to rinse it when the mordant has finished its action.

Muriatic acid is volatile. This property renders some caution necessary. A mordant containing muriatic acid must be well thickened, and the piece printed with it must not hang in a moist atmosphere, otherwise the muriatic acid escapes by the edges, and the pattern is destroyed.

Muriatic acid dissolves (löst) easily in water, and thus may still prove injurious to the dyed stuff; namely, in washing the printed pieces with caustic mordant. The acid may be made harmless, either by washing the pieces completely in running water, in which case the acid is quickly carried away, or it may first be passed through chalk water, and afterwards washed in the usual way.

The muriatic acid salts, as the muriate of alumina, cannot

be employed in printing, as they do not combine with the stuff like the acetates, but are washed away again by the process of rinsing.

Nitric acid.—This acid which also occurs in commerce, under the name of aquafortis, is of little use for the cotton manufacture. It is employed very seldom, and usually in such cases (nitrate of lead excepted) another acid would answer equally well; for example, nitrate of zinc, nitrate of iron, and nitrate of copper.

The two last answer as an addition in dark calico-printing, and their exhibition requires some caution.

When nitric acid of the usual strength is poured upon iron filings, red fumes are disengaged, the liquid boils, and, when the vessel is not very high, the whole passes over. What remains is a thick brownish red mass, which is quite useless. In this way nitrate of iron can be prepared. The mode of proceeding should be reversed, viz. instead of pouring the acid upon the iron, by introducing gradually some iron into the acid, and this is not to be repeated until all the iron previously added be dissolved. In this way the heat may be increased, although, by placing the vessel in cold water, the process is more securely performed. Good nitrate of iron should be dark-brown, and quite clear. Nitrate of copper and nitrate of zinc should be prepared with the same precaution. On a large scale it is more profitable, instead of copper and zinc, to dissolve copper chips and flowers of zinc in the nitric acid.

Of all the acids, nitric acid acts most injuriously on cotton; it should, therefore, be added in very small quantity to caustic mordants, and where it is possible its use should be dispensed with.

Acetic acid.—Strong acetic acid acts under no circumstances disadvantageously upon cotton fabric. Therefore, all metallic solutions, (mordants) when it can be done, are employed in the state of acetates. The acetate solutions are obtained partly by dissolving the metals directly in vinegar, as for example iron. This is allowed to get rusty in the air, and is then digested with vinegar. Since the efficient part of pyroligneous acid is also acetic acid, it may be employed for the same purpose instead of vinegar. This kind of solution, however, requires a long time.

Acetic acid dissolves lead foil, and forms sugar of lead or acetate of lead. It is a very useful salt, as will subsequently appear.

For calico-printing, some dye-woods are boiled with strong vinegar, instead of water. The boiling should not be continued long in this case, as much acetic acid will be driven off. It is better to dissolve the inspissated extract of dye-wood in vinegar.

The volatility of acetic acid just mentioned, exists in many of its combinations. If we have solutions of acetate of alumina, acetate of iron and acetate of copper, in an open dish, and place it in a warm place; a strong smell of acetic acid can be perceived during the evaporation of the salt, and at last a residue remains which is no longer soluble in water, as the portion of acetic acid required to dissolve it is removed. The same happens with these salts when they are placed on cotton instead of the dish. A part of the acetic acid flies off, and the salt with a little acetic acid remains upon the fibres.

Such compounds of acetic acid are, therefore, to be preferred for printing, which allow a portion of their acid to be driven off, as those already mentioned, as well as the acetate of tin. Acetates of manganese, zinc and chrome are not to be used, as they lose no acetic acid by evaporation.

The action of alkalies and of lime will be destroyed by vinegar. Hence, vinegar removes from dyed pieces all spots produced by the former. The brown spots which are produced by potash or lime upon Fernambuc-red, cochineal-red, quercitron-yellow and logwood-blue are completely removed by vinegar. The original colour generally returns when the fresh spot and the stuff is not yet washed. If it is so, the colouring matter will be washed out, and the early colour cannot again appear. Pyroligneous acid, which is impure acetic acid mixed with tar, may, when purified, be employed instead of acetic acid. It is used, however, in an impure, barely skimmed state, for the preparation of pyrolignate of iron; the formation of which is attended with difficulty in this respect, that it requires a long time.

Tartaric acid.—This acid occurs in commerce in transparent crystals, and is so readily soluble in water, that 100 lbs. of a saturated solution contain 64 lbs. tartaric acid.

This easy solubility affords a ready method of separating it from tartar, with which it may be adulterated, and which is very little soluble in water. If a solution of 1 lb of tartaric acid in 2 lbs. water, thickened with gum, be printed on cotton, and allowed to hang for 14 to 21 days at a temperature of 15° R (65°$\frac{3}{4}$) to 21° (79°$\frac{1}{4}$), we shall find that the printed places have suffered no injury, but are as strong as the unprinted portions. The printed specimen has, as at the beginning, a sharp border. Tartaric acid does not corrode the cotton, and attracts no moisture from the air, which would moisten it and produce a border. A strong heat in drying is injurious.

These properties render tartaric acid an excellent mordant, especially in the Turkey-red or purple manufacture.

A solution of nitrate of lead may be mixed in the solution of tartaric acid, without decomposition following. Hence, this forms a mordant of chrome-yellow upon madder-purple.

Tartaric acid decomposes chloride of lime by precipitating the lime and setting the chlorine free, which exhibits its bleaching action. Tartaric acid is, therefore, printed on a madder ground and dipped in a solution of chloride of lime; the printed parts become white, while the unprinted parts remain red.

The tartaric acid combines readily with potash, and forms tartar, or takes up a portion of potash from the sulphate of potash, forming tartar and bi-sulphate of potash. This last salt is very corrosive, and destroys the cotton like sulphuric acid; this fact deserves attention, and points out to the dyer, the rule never to bring tartaric acid and sulphate of potash together. This may sometimes happen without his knowledge, as in mixing Berlin or Paris blue not sufficiently edulcorated with tartaric acid, or in mixing tartar with sulphuric acid. In the last case, there is formed, not common sulphate of potash, which is not injurious, but bi-sulphate of potash.

Oxalic and citric acids.—Oxalic acid occurs in commerce in white crystals, which, if they are pure when held over a spirit lamp in a silver spoon, are completely dissipated. If a drop of oxalic acid falls upon cotton impregnated with the iron mordant, a rust yellow colour is produced, and a white spot remains upon the place. If this spot is allowed

to dry, and the cotton washed in pure water, and dyed in a solution of madder or logwood, it will be found that the spot produced by the oxalic acid remains white, while the ground takes up the madder or logwood iron colour. The oxalic acid thus dissolves the iron mordant combined with the cotton thread, so that it may be washed away in water. This property renders oxalic acid a very useful substance as a discharger for a fine pattern with an iron ground, which, after dyeing, appears white.

The oxalic acid salt, which is a combination of oxalic acid and potash, may also be employed;* yet on a very dull ground it acts with less certainty.

Citric acid answers the same purpose. It is best to employ both acids in conjunction.

Potash.—Most of the acids act injuriously upon cotton fabric, especially on the application of heat, but potash on the other hand does not produce any injury, when not allowed to act in too great excess, or too long. Thus a piece of cotton may be allowed to hang 3 weeks in a temperature of 68°, after being soaked in a solution of 1 lb of potash in 4 lbs. water, without any injury.

Hence, potash may be boiled with the raw cotton to prepare it for bleaching, without any disadvantage. For this purpose, it requires to be made caustic by abstracting its carbonic acid by means of lime. The proportion of lime to the potash depends upon the strength of the ley, which will be prepared according to the proportion of the water added. To make a very strong solution of caustic potash, much lime should be employed; for a weak solution, less will be required. A solution of potash which consists of 200 lbs. potash in 1400 water, requires from 56 to 60 lbs. of quick lime to make it caustic, while with half this quantity of water, we must take double the lime to obtain the same result.

For boiling the cotton, weak leys are employed. Hence, wood ashes may be used for their preparation, when they are cheap enough. For 1000 lbs. of good wood ashes, 50 to 60 lbs. of lime are required to render the potash contained in them caustic.

To determine if a ley is completely caustic, lime water may be employed. If after the addition of the latter, the

* Binoxalate of potash.—Edit.

ley remains clear, we may conclude that it is caustic; but, if a milkiness and flocky precipitate should appear, it is not caustic.

In order that we may not be deceived in this test, it is always necessary to drop the ley into the lime water, but not inversely the water into the ley. Some employ sulphuric acid for the same purpose, and consider a ley completely caustic, if, when dropped into dilute sulphuric acid, it does not effervesce. But when the effervescence proceeds from the carbonic acid, the latter being soluble in dilute sulphuric acid, this test is not quite certain.

Very strong caustic leys are prepared by boiling down weaker leys in an iron boiler, (whereby the ley must always be kept at a boiling temperature, or it will immediately absorb carbonic acid from the air), or from the ashes of the soap boiler. It is allowed to dissolve the alumina in the exhibition of the alkaline aluminous mordant, and for the production of manganese bistre. Caustic leys dissolve oil, forming a clear solution, and producing soap. If, on the other hand, the ley is not caustic, it combines equally with the oil, but the solution is milky and the oil readily separates again. This combination (oil mordant) is of great use in dyeing Turkey-red, as will subsequently appear.

Potash cannot be printed upon cotton any better than caustic ley. In the first place, the thickening is effected with difficulty, although, to a certain extent, it succeds well with strong gum. Secondly, potash has a great tendency to extract water from the air, and deliquesce. The printed pattern, therefore, has not a regular edge, but a border is formed.

(To be continued.)

ARTICLE VI.

Action of Ammonia and Muriatic Acid.
By ROBERT J. KANE, M.D.

(Extract from a Letter to Dr. Thomson.)

THE theory of the Amides which has been found of such interest in organic chemistry, induced me to study the action of ammonia upon various metallic compounds, and has led

me (I have good reason to believe) to the result, that a great number of compounds, hitherto considered to contain ammonia itself, do in reality contain the body $N\,H^2$, to which I have proposed to give the name *Amidogene*, reserving the word Amide to express its combinations. I have concluded the examination of the action of ammonia upon the Haloïd compounds of mercury, and purpose following up the re-actions on the other metallic bodies according as I can make time. The memoir on the mercury compounds is now before the Royal Irish Academy; but I take this opportunity of putting the results into your hands.

The popular idea of white precipitate is certainly wrong. In place of being $\ddot{H}g + Cl\,N\,H^4$, it cannot be proved to contain any constant quantity of oxygen at all. I analyzed it a great number of times, and the average of all the analyses, none of which diverge far, is

$$78\cdot60 = Hg$$
$$6\cdot77 = N\,H^3$$
$$13\cdot85 = Cl$$
$$0\cdot58 = \ddot{H}$$
$$0\cdot20 = \text{Loss, the water evidently}$$
$$\overline{} \quad \text{from partial desiccation.}$$

The formula I deduce is this, $(2\,Cl + Hg) + (2\,N\,H^2 + Hg)$ which perfectly explains all the properties of the body.

The white powder is acted on definitely by water or by alkalies in excess; the result is a yellow powder, having the composition $(2\,Cl + Hg) + 2\,\ddot{H}g + (2\,N\,H^2 + Hg)$ and sal-ammoniac is dissolved.

The compound $(2\,Cl + Hg) + N\,H^3$ is decomposed by water into white precipitate and sal-alembroth, as can be at once seen from their respective formulæ.

It is generally said that ammonia liberates black oxide from calomel; that is not true. There is abstracted but half the chlorine, and there is produced a dark gray powder having the composition $(Cl + Hg) + (N\,H^2 + Hg)$. The direct combination $(Cl + Hg) + N\,H^3$ gives this powder and sal-ammoniac by the action of water.

The iodides and cyanides form only the combinations $(2\,I + Hg) + 2\,N\,H^3$ and $(2\,Cy + Hg) + N\,H^3$, which are decomposed by water, without their elements entering into any new state of combination.

My analyses of ammoniuret of mercury agree closely with that of Guibourt; but dried at a moderate heat, I have found it to contain much water, which he does not mention as a constituent.   Its formula is (3 $\ddot{Hg}$ + 2 N H ^{3} + 4 $\dot{H}$) or, (2 $\ddot{Hg}$ + 2 N H ^{2} + Hg) + 6 $\dot{H}$), if we suppose the ammonia to exist as amidogene, which is almost proved by the composition of the chlorides of mercury compounds.

I expect to find still more remarkable examples among the compounds of those metals with small atomic weights. The large quantity of mercury in the bodies above described renders the differences in the proportions of the other ingredients too small to be decisive crucial instances.

Another subject on which I have got some interesting results is the action of Muriatic Acid on some oxygen salts. The sulphates are those I have most examined as yet. Dry muriatic acid gas is not absorbed by the sulphates of potash, soda, zinc, or magnesia; but sulphate of copper absorbs one atom, and the sulphates of zinc and mercury about half an atom each. The compound $\ddot{S}$ + $\dot{Cu}$) + Cl H is brown, absorbs water rapidly, and gives, by crystallization, chloride of copper, all the sulphuric acid remaining in the liquor. The action of liquid muriatic acid is also interesting, it expels all the sulphuric acid from bluestone, forming chloride of copper. It takes half the base from the sulphates of potash and soda, and hence, in the latter case the large quantity of suddenly liquified water, which gives to that re-action the power of acting as a freezing mixture.

The results, of which the above are but a few, have led me to a means of examining how far solution is accompanied by chemical decomposition, a subject on which I am just now engaged.

Article VII.

Notice of some Recent Improvements in Science.

HEAT AND LIGHT.

1. *Temperature of the Globe.*—M. Poisson, in his elaborate work entitled *Mathematical Theory of Heat*, has broached

some new notions in respect to the source of the earth's heat. He observes, that the spherical form of the earth, and its flattening at the poles, prove that it was originally in a fluid, or perhaps in a gaseous state. After this period, it can only have become solid, either wholly or in part, by a loss of heat, proceeding from the circumstance that its temperature exceeded that of the medium in which it was placed. He conceives, that it has not been demonstrated that the solidification commenced at the surface, and gradually extended to the centre, as those theorists assert who adopt the idea of a fluid centre. The contrary appears to Poisson more probable ; those portions nearest the surface having been cooled first, have descended into the interior, and been re-placed by matter from the interior, which has again descended in its turn, and thus the process was repeated until the whole mass was cooled down. But further, the central layers would become solid, in consequence of the immense superincumbent pressure at a temperature equal to, or even superior to that of the layers nearer the surface. Experiment has proved that water at common temperatures, when submitted to a pressure of 1000 atmospheres, undergoes a condensation of about $\frac{1}{20}$ of its original volume. Now, if we conceive a column of water equal in height to the earth's radius, and reduce its weight to one half of what it possesses at the surface, in order to render it equal to the mean gravity of each radius of the earth, supposing the latter homogeneous ; the inferior layers of this liquid column will undergo a pressure of above three millions of atmospheres, or equal to above three millions of times that which reduced the water $\frac{18}{38}$ of its volume. Without any knowledge of the laws of the compression of this liquid, we must still believe, that such an enormous pressure would reduce the inferior layers of the mass of water to the solid state, even when the temperature was very high.

In order to explain the elevation of temperature which we observe, in proceeding from the surface towards the centre of the earth, he suggests the effect of the inequality of the temperature of the regions of space, which the earth successively traverses, because he considers it very improbable, that the temperature of space is every where the same. The mean temperature of space, may be admitted to differ

little from zero, in place of being, as has been generally
calculated below the temperature of the coldest regions of
the globe.   The variations in the temperature of space
may, however, be very considerable, and they ought to pro-
duce corresponding variations in that of the earth, which
will extend to depths dependant on their extent and degree.
" If we suppose for example, a block of stone to be carried
from the equator to our latitudes, its cooling will have com-
menced at the surface and extended into the interior, and
if it has not reached the whole mass because the period has
been insufficient; this body when it has arrived in our cli-
mate will present the phenomenon of a temperature increas-
ing from the surface.   The earth is in the condition of this
block of stone; it is a body which proceeds from a region
whose temperature was superior to that of its present situa-
tion; or, if we wish, it is a thermometer, moveable in space,
which has not time, in consequence of its great dimensions
and its degree of conductibility, to take in through its whole
mass the temperature of the different regions which it tra-
verses.   At present, the temperature of the globe increases
below its surface.   The contrary has already, and will again
take place.   At other periods, besides at epochs separated
by numerous ages, this temperature ought to be, and will
be, by consequence, much higher or much lower than it is
now, which prevents the earth from being always habitable
by the human species, and has, perhaps, contributed to the
successive revolutions of which its external layer has pre-
served the traces."*

   2. *Theory of heat and light.*—Ampere, in stating his views
in reference to a theory of heat, sets out with defining *par-
ticles, molecules*, and *atoms* which he considers to enter into
the constitution of matter.   A *particle* is an infinitely small
portion of a body, and of the same nature with it, so that a
particle of a solid body is solid, that of a liquid body liquid,
and that of a gas aeriform.   The *particles* are composed of
molecules kept at a distance :   1. By what remains at this
distance, of the attractive and repulsive forces peculiar to
the atoms; 2. By the repulsion which the vibratory motion
of the interposed ether establishes between them : and 3.
By the attraction directly proportional to the masses, and

* Bibliotheque Universelle, June, 1835.   Ann. de Chimie, lix. 71.

inversely as the square of the distance. *Molecules* consist of a collection of atoms kept at a distance by attractive and repulsive forces peculiar to each atom. *Atoms* are material points from which these attractive and repulsive forces emanate.

From this definition, it follows, he considers that a molecule is essentially solid, whether the body to which it belongs be solid, liquid, or gaseous; that the molecules are polyhedrons, of which these atoms, or at least a certain number of these atoms occupy the summits, and it is these polyhedrons that are termed primitive forms by crystallographers. The particles alone can be separated by mechanical means. The force which results from the vibrations of the atoms may separate the compound into simpler molecules. Chemical action can alone separate the latter. Thus, in detonnating a mixture of 1 volume of oxygen and 2 volumes of hydrogen, by which 2 vols. of vapour of water are formed, each molecule of oxygen is divided into two, and the atoms of each of these halves unite with the atoms of a molecule of hydrogen to form a molecule of water. Proceeding upon these premises, Ampere distinguishes the vibrations of molecules from those of atoms. In the first, the molecules vibrate together, approaching and retreating alternately the one from the other, and whether they vibrate in this manner or remain at rest, the atoms of each molecule vibrate, and in fact, always do vibrate by approaching and retreating the one from the other alternately, without ceasing to belong to the same molecule. The latter, he terms atomic vibrations. To the vibration of the molecules, and to their propagation in the surrounding media he attributes all the phenomena of sound; to the vibrations of the atoms he ascribes all those of heat and light.*

3. *Optical properties of Charcoal.*—If a portion of well burned fir charcoal be placed upon a layer of heated coal on a wind furnace, and all openings be closed, so that no air can penetrate below the coal, the combustion will be carried on entirely by the decomposition of the carbonic acid. After the fire has subsided, Degen found that the portion of coal had wholly or in part dissolved into a mass of fibres, which did not adhere strongly to each other.

* Ann. de Chim. et de Phys. lviii.

When examined under the microscope they were found to be round tubes; they are more or less translucent, and their colour by transmitted light is brownish yellow. These tubes have round apertures on their sides, whose margins are thicker than the rest of the sides; some of them when of a large size, however, have no edges of any considerable diameter. When heated to whiteness in platinum foil before the blowpipe, these tubes lost their translucency and became very brittle. The diameter of these tubes was from about ·00049 inch to ·0000908 inch. There is a remarkable appearance observed when the miscroscope is directed through one of the apertures upon a distant (*entfernten*) object. This object appears double. One of the figures stands *upright* about ·0004 behind the opening; it is, at least so distinct, that we can see the window-post clearly. The second figure is inverted, and appears before the opening; it is more indistinct than the first. These appearances belong to the phenomena of diffraction. The form which the charcoal assumed, by the powerful heat applied in the manner described, is similar to the filamentous matter examined by Dr. H. Colquhoun, which was obtained during some trials made by Mr. Macintosh to convert iron into steel, by surrounding it with coal gas in an air tight iron chest.*

ARTICLE VIII.

ANALYSES OF BOOKS.

I.—Philosophical Transactions of the Royal Society of London for 1835, Part II.

(*Concluded from* page 149.)

ANATOMY AND PHYSIOLOGY.

Continuation of the paper on the relations between the nerves of woolton and of sensation, and the brain; more particularly, on the structure of the medulla oblongata and the spinal marrow. By Sir CHARLES BELL, F. R. S., &c.

THE investigations detailed in this paper refer to the structure of the spinal marrow, and its relations to the encephalon on the one hand, and to the origin of the nerves on the other. If, after having laid bare the medullary columns of the spinal marrow, we split up the

* Poggendorff's Ann. xxxv. 468.—Thomson's Inorganic Chemistry, i. 160.

columns, we shall find that their surfaces are covered with cineritious matter. If we now clear away the cineritious matter from the columns below, we shall first discover the two lateral tracts or columns regular as nerves. The columns, when divested of their cineritious matter, are found to be covered with a succession of coats, the superficial layers furnishing the coats of the higher nerves, and the lower layers going off into the roots of the nerves, as they successively arise. The sensitive or posterior roots of the spinal nerves disperse in the substance of the lateral columns, and are not derived from the cineritious matter as some assert. Between the lateral columns the cineritious matter lies deep, upon raising it the anterior or motor columns are seen, which resemble the lateral columns. Such are the general features of the spinal marrow. If, returning the' parts to their places, we now raise the two posterior columns, we find them diverging at the back of the medulla oblongata, and forming the triangular space of the fourth ventricle. Each of these columns is now seen to consist of two, the outermost the larger, and that towards the central line the smaller, and in shape pyramidal. Following them up, they are recognized to be the processus cerebelli ad medullam oblongatam. If now, we trace the cineritious matter on the lateral columns, we can follow it into the 4th ventricle, and, indeed it constitutes one sheet of matter from the cauda equina to the roots of the auditory nerves, and forms a grand septum between the anterior and lateral parts of the spinal marrow, which belongs to the cerebrum, and the posterior columns which are related to the cerebellum.

Union of the lateral columns in the medulla oblongata.—On removing the cineritious matter from the cerebral position of the spinal marrow, the two lateral positions are seen upwards or towards the brain, each of these columns has a double termination, first, in the root of the fifth nerve, and secondly, in the union of the columns, that is, their decussation. These columns lie separate in the spinal marrow ; but at the medulla oblongata, they form one round column rather less than half an inch in length. As it ascends, they are disentangled, but do not separate, and they consitute processes of the cerebrum running down from the' back of the crura cerebri.

The septum which divides the right and left sensitive tracts, as seen in the 4th ventricle, and whose nature is still a desideratum, splits to permit the decussation of the columns. When a transverse section is made we observe the motor columns approaching the sensitive columns, but no union takes place.

Observations on the theory of Respiration. By William Stevens, M. D., &c.—This paper commences with stating, that the cause of the dark colour of the venous blood has long been a subject of discussion, and even, at the *present* moment the question has not been satisfactorily decided. We perfectly agree with this affirmation, and cannot discover in what respect Dr. Stevens has assisted in throwing more light upon the subject. We have elsewhere, (*Records,* vol. i. 56.) stated the result of Vogel, who succeeded in extracting carbonic acid

from blood, under the exhausted receiver of an air pump, and also, that of Brande, who obtained 2 cubic inches of acid from 1 ounce of both arterial and venous blood. Dr. Davy could detect no such evolution, and Gmelin and Tiedemann could not succeed unless after the addition of acetic acid. Now, what conclusion should be drawn from this variety of results? Two only appear to us admissible, either, that blood, in some cases, does contain carbonic acid, and in others does not; or, that the experiments had been performed under different circumstances. According to Dr. Stevens, no carbonic acid is given out from venous blood by the mere removal of pressure; but, when two vessels are employed (one filled with hydrogen, and containing some blood, and communicating with another by means of a bent tube dipping into barytes water), and then the whole placed in an exhausted receiver, so that the hydrogen and whatever gas may be evolved from the blood passes through the alkaline solution, he found that a precipitate was always produced in the latter. We have no proof, it is to be regretted, from the mode in which the experiment is detailed, that the hydrogen contained no carbonic acid, or, that the precipitate was a carbonate. But there is nothing new in all this, for Vauquelin, long ago, stated, that blood, placed in hydrogen, evolved carbonic acid. We observe, that the author claims the discovery of the diffusive power of oxygen in reference to other gases, as exemplified in the case of hydrogen in the experiment described by him. But the fact is, that Mr. Dalton, long ago, broached the subject of the mechanical mixture of the gases generally, while Mr. Graham discovered the law of their diffusibility, viz., by the interchange of indefinitely small volumes of the gases, inversely proportional to the square root of their densities.

Having proved that carbonic acid exists in venous blood, the author concludes, that the latter derives its dark colour from the presence of that acid. Now, what can be more fallacious than such a conclusion, until it has been determined that arterial blood contains no carbonic acid? especially, when, according to Mr. Brande's experiments, it appears that from both kinds of blood carbonic acid can be extracted. The author then proceeds to support the common theory of respiration by the absorption of oxygen, and the evolution of carbonic acid by the venous blood in the lungs. Some of the hypotheses, which he brings forward are amusing enough. For example, he supposes that the globules, which are observed to leave the tissue surrounding the extreme arteries after the blood has left the latter, are minute particles of oxygen, and that the globules which return are minute particles of carbonic acid; but judiciously adds "this cannot be easily proved." Such speculations might have suited the pages of some ephemeral publications, but should not have been introduced into those of the Philosophical Transactions.

Discovery of the Metamorphosis in the second type of the Cirripides, *viz., the* Lepades, *completing the natural history of these singular animals, and confirming their affinity with the* Crustacea. By J. V. Thompson, F. L. S.

*On the double Metamorphosis in the Decapodous Crustacea,
exemplified in Cancer Mænas.* By J. V. Thompson, F. L. S.
On the supposed existence of Metamorphoses in the Crustacea.
By J. O. Westwood, F. L. S., &c.—These papers are devoted, the
two first to the illustration of a remarkable discovery made by the
author, and the other to a refutation of the inferences deduced by
Mr. Thompson from certain facts to which he has been eye witness.

According to the latter, the crustacea undergo a metamorphosis, a
change not consisting merely in the periodical shedding of the outer
envelope, but by which certain organs are acquired. After keeping
a full grown zoe for more than a month, it died in the act of chang-
ing its skin, and of passing into a new form, but one by no means
similar to that expected; for its disengaged members, which were
changed in number as well as form, corresponded with those of de-
capoda (crabs, &c.) viz. five pair, the anterior of these furnished with
a large claw or pincer, and, from being natatory and cleft, became
simple and adapted to crawling only. He found also, that the com-
mon lobster undergoes metamorphoses, but less in degree; the change
being from a cheliferous schizopode to a decapode, its first stage being
a modified zoea with a frontal spine, a spatulate tail, and wanting
sub-abdominal fins, " in short, such an animal as would never be
considered what it really is, were it not obtained by hatching the
spawn of the lobster." He hatched the ova of the common crab
(*cancer pagurus*), which presented exactly the appearance of the
Zoea Taurus with the addition of lateral spines to the corslet.
Numerous genera are subject to similar metamorphoses, among
which Mr. Thompson enumerates *Pagurus, Porcellana, Galathea,
Crangon, Palemon, Homarus, Astacus.*

Mr. Westwood, in endeavouring to refute the conclusions of the
author, objects to the vagueness of his descriptions, and asserts, that
in the plates which he has given (which he allows to be beautiful)
of the metamorphoses of the zoes into crabs, there is no change in
reality,—that the zoe has not lost a single character which it possessed.
2. That the appearance of the limbs (represented by Mr. Thompson
as perfectly disengaged), is totally at variance with the principles
of ecdysis. 3. Mr. Thompson states, that his large zoes differed
from the smaller ones in the greater degree of developement of all
their organs. This is precisely what *would* happen if the large zoes
were perfect animals, and precisely what would *not* occur if the zoes
were incipient crabs. 4. The elongated tail, rostrated cephalothorax,
but especially the structure of the mandibles, and two pairs of
maxillæ, peculiarities of the zoes, and so evidently partaking of the
macrourous type, tend to negative the opinion, that they would ever
become brachyurous. 5. The cray fish, according to Rathke, undergoes
no metamorphoses. Hence, Mr. Westwood considers he is warranted
to conclude, that the other decapods likewise undergo no change.

Mr. Thompson has extended his researches also to the *Cirripedes,*
and has come to the conclusion, 1. That the *Cirripedes* do not con-
stitute a distinct class of animals, as they have been considered by
all late naturalists, being connected with the *Crustacea Decapoda*
through the *Balani,* and with the *Entomostraca* by means of the

Lepades. 2. That they have no relation whatever with the *Testacea*, as supposed by Linnæus and the older systematists. 3. That the *crustacea*, now, therefore, furnish examples of a class in which we have animals free and fixed, with eyes and eyeless, and with the sexes separated in some and united in others, all of which are characters, to which, naturalists have attached the greatest importance, as regards classification. 4. That the proof of metamorphoses being fully and satisfactorily established, tends still to maintain the affinity so long recognized between the *crustacea* and *insecta*.

His description of the change in the Cancer mænas is striking, and deserves to be noted, as it details another change which crabs undergo, and cancels another genus in natural history. He had formerly found, that the young of the common market crab (*cancer pagurus*) first presents itself as a zoe, and that a full grown zoe passed into some other more perfect form. He has since ascertained that this state must have been that of a *megalope*. He ascertained this fact by keeping a number of individuals of a *megalope* in regularly renewed sea water. These began after a short time to change into a minute crab, until the whole of them, about two dozen in number, were so changed. The *Cancer Mænas*, he finds in its first or zoe stage, is wholly natatory from structure, while in its second it occasionally walks by means of its thoracic members, now become simple; but more commonly swims by the motion of its sub-abdominal fins, which are greatly developed for this purpose. In both stages it is, therefore, a *macroura*, but only in the latter, evidently, related to the *decapoda*.

Mr. Thompson concludes his papers by noticing the indifference of our zoologists to these important discoveries, and the activity of our French neighbours in respect to the subject. The Institute deputed two naturalists to spend a summer at Isle Ré to make observations. The report of Milne Edwards, one of them, was unfavourable to the statements of Mr. Thompson. He pronounced that the crustacea were hatched with the form and structure of their adult parent. Is the matter to rest here? We can discover no reason why it should. Mr. Thompson, we believe, to be an accurate observer. We believe Mr Edwards to be the same; but there cannot be a doubt, that the former has possessed infinitely better opportunities for scrutinizing the phenomena in question, than the latter, or, perhaps, than any other naturalist. The satisfactory mode of deciding the matter would be, that Mr. Thompson should exhibit facts similar to those he has described to the most competent persons for appreciating them, and we have no doubt that some of our naturalists, in the course of the ensuing summer, will visit the scene of Mr. Thompson's labours (Cork) with this object in view.

Remarks on the difficulty of distinguishing certain genera of Testaceous Mollusca by their shells alone, and the anomalies in regard to habitation observed in certain species. By John Edward Gray, F. R. S., &c.—In this paper the subject is considered under two views. 1. In reference to shells apparently similar, but be-

longing, on a comparison of their animals, to very different genera.
2. Of species belonging to the same natural genus inhabiting essentially different situations.

I. The shells of the genera *Lottia* and *Patella* are so extremely alike, that Mr. Gray has not been able to find any character by which they can be distinguished with any degree of certainty, yet their animals are extremely dissimilar. The internal structure of the shells likewise agrees. Yet the animal of the *Patella* has the branchiæ in the form of a series of small plates disposed in a circle round the inner edge of the mantle, while that of *Lottia* has a triangular pectinated gill seated in a proper cavity formed over the back of the neck within the mantle, agreeing in this respect with the inhabitants of the *Trochi, Monodontæ,* and *Turbines,* from which it differs so remarkably in the simple conical form of its shell. Similar instances of difficulty in forming distinctions occur in *Pupa and Vertigo, Vitrina and Nanina, Rissoa and Truncatella, Siphonaria and Ancylus, Littorina and Assiminia,* among the univalves. The *Mytilus polymorphus,* a fresh water species, does not differ, as far as the shell is concerned, from any of the *mytili;* but the animal is quite distinct. In the *mytili* the lobes of the mantle are free throughout nearly their whole circumference, while in the *M. polymorphus* they are united through nearly their whole extent, leaving only three small apertures, one for the passage of the foot and beard, and the other two for the reception and rejection of the water. It must, therefore, form a new genus, which has been termed *Dreissena* by Van Beneden. The *Iridinæ* and *Anodontæ* possess similar shells, but quite different animals; so also *Cytherea, Artenies, Cyclas,* and *Pisidium.*

In many of these univalves it is impossible to distinguish the genera without attending to the opercula, as in *Paludinæ* and some species of *Littorina, Phasianella,* and *Neritinæ* as distinguished from *Neritæ.* The genera of *Bullia* and *Terebra* are distinguished by the lip in the former being large and expanded, and in the latter small and compressed, occasioned by the different shapes of the feet; the former has also large and eyeless tentacles; the *Terebræ* have small and short tentacles, bearing the eyes near the lips. A similar difference exists between *Rostellaria and Aporrhais (Strombus pes pelicani).*

II. The difficulties in this division the author considers under four heads. 1. Where species of the same genus are found in more than one kind of situation, as on land, in fresh and in salt water. 2. Where one or more species of a genus, most of whose species inhabit fresh water, are found in salt or brackish water. 3. Where, on the contrary, one or more species of a genus, whose species generally inhabit the sea, are found in fresh water; and, 4. Where the same species is found both in salt and fresh water.

1. *Auricula scarabus* and *A minima* occur in damp places on the surface of the earth; *A Judæ* sandy places overflowed by the sea; *A myosotis, A coniformis, A nitens, &c. (Conovulus),* in the sea; *A Dombeyi, A fluvialis (Chilina),* in fresh water.

2. Lymnaceæ commonly occur in fresh water; but *L Balthica* is

found in brackish water on the shores of Gothland and Scania, and *L succinea* in the sea near Trelleborg. The Neritinæ are fresh water shells, yet *N Viridis* is a marine species at Martinique; *N crepidularis* in salt water lakes; *N meleagris* amphibious; *N auricula* at Bourbon; *N Pupa* probably in the sea; *Melania amarula, M fasciolata, M lineata* fresh water in India; *M Oweni* brackish water.

3. The *Aplysia dolabrifera* occurs in almost fresh water in marshes in Bourbon; *Cerithium sulcatum* brackish water; *Cerithium reticulatum* fresh water of Florida Keys; *Bulla Hydatcs* brackish pools in Chili; *B fluviatilis* mud of the Delaware; *Littorina fusca (Paludina Pfeif)* and *L naticoides* fresh water.

4. *Tellina solidula* occurs in the brackish water of the Baltic; *Mya margaritifera* in nearly fresh marshes in Bourbon; *Mya arenaria* high up in rivers; the common oyster, it is said, can flourish in fresh as well as in salt water; a large group is preserved in the museum of the Bristol institution said to have been dredged up in a river on the coast of Africa, where the stream was so sweet as to have been used to water the ship. Mr. Say found the *Neritina Meleagris* inhabiting St. John's river, in E. Florida, from its mouth to Fort Picolata, a distance of 100 miles, where the water is potable; M. Rang found *Neritina auriculata* in fresh and salt water; the *Ampullaria ovata* inhabits Lake Mareotis and fresh water lakes in the oasis of Siwah. The common cockle *(Cadium edule)* is observed in the ditch of brackish water at Tilbury Fort.

From these facts Mr. Gray concludes that the general rules, which have commonly been regarded as decisive of the localities inhabited by recent shells, and of the nature of the deposits in which the fossil species are found, cannot safely be employed for practical purposes without considerable reservation.

II.—*Principles of the Differential and Integral Calculus familiarly illustrated, &c.* By the Rev. WILLIAM RITCHIE, LL.D., F. R. S., &c. 12mo. Taylor, London, 1836.

THOSE who are about to enter upon the study of this branch of the mathematics are indebted to the author of the treatise before us, for a boon which only those who have had to labour through a "science of symbols and algebraic formulæ without any illustration or practical application," can sufficiently appreciate. An attempt to adjust a science to the level of ordinary capacities might be made without success, unless the writer possessed a thorough knowledge of the subject, and the rare ability of penetrating into the causes which obstruct the reception of a new study in the human mind. Dr. Ritchie has shewn, not only in this work, but in many others, that he is endowed with both of these qualifications in a remarkable degree: and it is always a pleasant subject for contemplation to observe a philosopher anxiously interested about the progress of his successors, and at the same time instructing his contemporaries. The present work cannot fail to encourage the student to study the important science of which it treats, instead of filling him with "the doubts of

imperfect faith," which he must necessarily experience when he inspects the enormous pages of formulæ which may frequently meet his eye, and which, in numerous instances, can be of no benefit either to the student, or to him who has advanced into the depths of science ; because the former cannot understand them, and the latter would always prefer to make his own calculations, than to follow merely the footsteps of another.

It is no uncommon cause of the neglect of the study of the calculus that, it is always placed after the most intricate and difficult parts of algebra. The author, however, dissipates this error by shewing that, the differential and integral calculus may be readily comprehended after the pupil has acquired a knowledge of the elements of geometry and the principles of algebra, as far as the end of quadratic equations. He begins with an introduction explanatory of constant and variable quantities, infinity, &c. in such a distinct and familiar mode, that it is impossible to fail of understanding him as he proceeds. The object of the differential calculus, he observes, is to determine the ratio between the rate of variation of the independent variable, and that of the function into which it enters. This is illustrated, as is very properly done in every case, by a numerical illustration. Thus, " if the side of a square increase uniformly at the rate of three feet per second, at what rate is the area increasing, when the side becomes 10 feet ; $1 : 2x :: 3 : 6x$. Hence, the rate of increase is 6×10, or, at the rate of 60 square feet per second." He then explains the notation of Newton and Leibnitz, adopting, very properly, that of the latter ; as, for example, dx instead of $\dot{x}$ to denote the rate at which the variable quantity represented by x is increasing ; although we have no doubt that he yields the palm of accurate reasoning to the metaphysics of the former, and admits that of the latter to be much less philosophical.

In reference to the integral calculus, he observes, that a pupil will generally be at a loss to understand what is meant by finding the integral of a given differential. In plain language, it only means that " we have given a quantity which varies uniformly, and the ratio of its rate of variation with *another quantity* depending on it and given quantities, to find the value of that quantity." The 3d, 4th, 5th, and 6th sections are devoted to rules for differentiating and intigrating simpler forms of functions and differentials ; of expressions containing two independent variables ; of functions having general indices, and the reduction of differentials to known forms, integration by series and definite integrals. This concludes the first portion of the work.

The second part is taken up with the application of the preceding rules and principles to useful purposes, under the heads of maxima and minima of quantities, curves of the second order, which is particularly worthy of notice, from the concise and distinct mode in which their genesis and nature are treated of, normals and subnormals, length of arcs, areas of surfaces, and surfaces and capacities of solids.

Part third treats of the developement of algebraic expressions into infinite series, differentiation of transcendental functions and integra-

tion by logarithms and arcs of circles. The sections on logarithms are very valuable, and will follow well after the perusal of the introduction to the subject by Biot which we have given at the commencement of the present number. We confidently recommend this admirable treatise to the attention of mathematical teachers, who cannot fail to obtain from it, assistance of the most valuable description, in smoothing the way for the reception of the calculus by young minds.

ARTICLE IX.

SCIENTIFIC INTELLIGENCE.

I.—*On the Arrangement of Mineral Collections.*

THE notice, in your Number for January, of a new system of Mineralogy, &c. by Professor Thomson, will excite the attention of your readers: and I wish to profit by the occasion, to examine the relative advantages of the chemical and natural arrangement of mineral collections. My own experience, such as it is, has led to the preference of the latter mode ; and the object of this paper will be, to give, in very general terms, the reasons for that preference : reserving details for a more advanced stage of the discussion, into which I wish to draw either Professor Thomson, or one of his qualified pupils who may have more time to spare.

In the case of private collections, there may be different motives for preferring the one or the other; or, what is rather more common, something between the two. The miner would prefer the analytical order ; the lapidary the natural ; the virtuoso would spare himself the labour of a strict arrangement, by adopting the intermediate or compound ; and the naturalist, perhaps, a divided system ; arranging chemically those species which depend, for their scientific or practical interest, on their elementary ingredients ; and according to their physical properties, those which owe their interest chiefly or altogether to those properties. For mineralogy, though so intimately connected with chemistry, is certainly a main branch of natural history.

But, whatever may be the inducement to the particular mode of arrangement of a *private* collection, of *public* ones it may be assumed, that their chief purpose is to facilitate the knowledge of mineralogy ; and on this ground it is that the chemical system seems to me objectionable.

I. From the inaptitude of its grouping to produce impressions of general similitude and distinction ; descending through divisions and subdivisions, and thus facilitating the discrimination of individual species.

II. From the uncertainty of analysis, in the present state of science, as a distinction between mineral species.

III. From its fixing our attention upon analytical distinctions, and thus diverting it from those upon which we must depend, for distinguishing one mineral from another.

It may be proper to exemplify the application of these objections.

I. Is it not the just complaint of almost every student, that he finds his greatest *difficulty* to be the attainment of the power of distinguishing one mineral from another ; so as readily to ascertain the name or nature of any specimen which falls in his way ?   Until he has attained this power of distinction, to what purpose can he avail himself of the knowledge of names, classification and composition? And is it not the experience of persons who have made greater advances, that this practical difficulty once conquered, the acquisition of the science becomes rather a recreation than a labour ?   It is true that, in the chemical arrangement, having the minerals before us, we can compare their natural properties ; but it must often be done by detail, and learned, as it were, by rote.   Whereas, by bringing together into orders those which have certain degrees of general resemblance ; and sub-dividing again, into genera, those having the closest affinities ; the points of discrimination for the species are contrasted within small compass.   The mind soon acquires the leading characters of the orders ; and those of the genera are pretty readily remembered, taking the most characteristic species as the type of the genus.   Thus, the species become assorted, in the memory, into small groups ; where their distinctive characters are perceived, with a degree of facility, very different from the complicated selection and comparison of individuals from amongst the 500 species comprised in our systems ; where those having the closest resemblances will sometimes be widely dispersed, and vice versa.

Suppose the student to have a well characterized specimen, of which he wants to know the name.   He finds it easier to ascertain with the aid of a collection, even chemically arranged, than by reference only to his books.   But he will be likely to find specimens more or less resembling it, in different parts of such a collection, without any efficient guide to the specific distinctions by which he may readily decide between them.   But in the natural arrangement he would seldom be long without perceiving the order and genus to which his specimen appertained ; and thus the points of destinction are brought (as above observed), into close contrast. If I am not in error, practical mineralogy would be acquired in Mr. Allan's collection, in half the time it would need in the British Museum.

It is not to be denied that the natural arrangements has its hitherto unsurmounted difficulties and imperfections, which may be adduced by the advocates for the chemical system ; but I apprehend they will be found both fewer and less important in the former than in the latter.

II. As to the second objection.   The uncertainty of analysis, as a specific distinction, interferes in several ways.   1. The doubtful accuracy of the analyses themselves.   2. The result of Isomerism ; rendering substances analytically identical, different mineral species. 3. Isomorphism, whereby substances closely approximating, or even identical in mineralogical character, may be dispersed in a chemical system.   4. The difficulty of deciding, upon fixed principles, under which genus to place, and consequently, where to find certain complex minerals.

1st. In the first case, taking up a mineralogical book, and observing

the analyses of the same mineral, (as there recorded) by different chemists; how much do they sometimes differ; and how differently will they be placed, according to our confidence in one or other of the authorities. How many minerals, too, are there, of which we have still reason to suspect the recorded analyses as erroneous; and, how frequently does the detection of such errors subject us to the annoyance of transferring a specimen, both in the collection and the catalogue. But, 2d, (supposing the analyses to be correct), will the same elements, united in one instance by igneous, in another by aqueous action, form the same mineral species? In the case of aragonite and calc spar, (and others might be quoted, if we could depend upon the published analyses), isomerism is produced by the same action (aqueous) on both sides; and we have yet all to learn of the causes and extent of this phenomenon in mineralogy; of which, however, the incovenience in the chemical system will not be great. But it is otherwise with the 3d case, Isomorphism, the micas, the hornblendes, the garnets and several other species, mineralogically all but identical, must be chemically dispersed in different parts of the collection; and some will apply to the 4th case; their proper places, upon fixed principles, being difficult to decide.

III. My third practical objection to chemical arrangement, that by diverting our attention from the physical distinctions, minerals are mistaken for one another, is of not less importance. How many instances may be referred to of the analyses of one mineral being recorded under the name of another, by chemists of the first eminence. And, in your Number for December, a foot note at page 445 makes it appear that a specimen examined by Berzelius, and given to him by Haüy, was a different mineral from what both those philosophers understood it to be. Now, their mineral systems, although differing in arrangement, are both chemical, and, therefore, subject to this charge, of fixing the attention on other than the natural distinctions. And of a method which leaves the greatest masters liable to mistake their specimens, when deliberated, noticed for analyses, or examination for the purpose of publishing their distinctive properties; how can the *student* expect such a method to facilitate his attainment of the power of recognizing and distinguishing them?

For such reasons, which may be amplified on a future occasion, the natural appears to me preferable to the chemical arrangement of a public mineral *collection*. But a chemical arrangement, on *paper*, as an index, or catalogue raisonnée, symbolically elucidated, and succinctly explained, I conceive to be an accompaniment of essential importance in a scientific point of view. Such a book was wanted in our language, and the first volume of Dr. Thomson's work seems just to supply the deficiency. It may be applied to any collection by putting in, with the pen, the order and genus in the margin of each article, both in the synopsis and the text; making, at the same time, responding references in the catalogue of the collection to the class and genus of the book. Thus, I think, the practical and scientific progress of the student would be at once promoted; and the knowledge of mineralogy facilitated in a degree much beyond what it has yet reached in this country.   P.

P. S. Although not disposed to mix up the discussion of symbols with the present subject; yet, having mentioned the importance of symbolic explanation, it becomes right for me to add, that those introduced in the work quoted possess neither the analytical explicitness of the chemical symbols of Berzelius, nor the perpetuality of his mineralogical ones; which, recording the elements only as proportionals to the units of oxygen with which they are combined, are independent of theory, and express the same compound in all times and countries, under all possible differences of atomistic hypothesis.

So long, however, as they are confined to the book, being explained by it, it is only one lesson the more for us to learn. But if they go beyond it; and particularly, if they are to be, as they have been, adopted in your Records, to which we look for our knowledge of the progress of foreign chemistry; and where, consequently, one set may be found clashing with another; may not the innovation be alike injurious to the cause of science, and to the value of your labours?

Note.—I have only space to observe that the reasoning of my ingenious correspondent does not convince me of the advantage of making the chemical subservient to what is termed (I think improperly) the natural system of arrangement; because, 1st. The external characters of minerals depend upon the nature and mode of combination of the elements of which they are composed; and, therefore, by looking to the former for a method of classification instead of to the latter, we commit the logical inaccuracy of taking as the basis of the system, an effect instead of a cause. 2d. Because I consider minerals to be nothing else than salts; and none have ever attempted to arrange these according to their external properties, the only sure test of their nature being chemical analysis; and, 3d. Because a mere knowledge of the external characters of compounds is useless, and can only be considered as subservient to the grand object, of the pursuit of mineralogy, viz., a knowledge of the chemical properties of matter. The remark of Rose in the note alluded to (*Records*, vol. ii. p. 445.) obviously implies, that either Haüy or Berzelius committed a mistake.—EDIT.

II.—*Anecdote of a Bee.*

IN a letter to the Editor, Mr. Tomlinson, of Salisbury, says, "I am fond of bees, and keep a few hives. I have several curiosities respecting them. The results of hours of attentive watching, but I can only venture to describe one, and I do not know if it has ever been noticed before. On the 13th of May last, I was observing my bees, and one had just returned from his flight for provender, and was covered with pollen. He pitched about a foot from the entrance of the hive, and, in hastening on, caught his leg in a crack of the stool just wide enough to hold it securely, and his pulling only served to fix the limb tighter. He was evidently annoyed, and for a few minutes tried various means of extrication, and at length seemed to get impatient, especially as other labourers were fast pouring in with their loads. He then began leisurely and systematically to turn

himself round several times, and literally twisted off his leg at the third joint, leaving the limb in the wood.   He then moved into the hive, as if nothing had happened.   This is the only instance, I have seen, or have ever read, or heard of, of the spontaneous self mutilation of an insect, but as I am no naturalist, the fact may be well known.   At any rate, if you think it worth any thing, you can make what use of it you please, and I pledge myself as to its accuracy."

III.—*Calcarco Sulphate of Barytes.*

This mineral was discovered among the debris of the abandoned lead mine of Nuissiere near Beaujeu, by the Marquis de Dree, and hence, has been called, *Dreelite*, (*Ann. des Mines*, viii. 237.)   It consists of white, pearly, rhomboidal crystals without any modifications..   It possesses a triple cleavage parallel to the faces of a rhombohedron. Its primitive form appears to be an obtuse rhombohedron, the dihedral angle of which may be about 93° or 94°.   It, therefore, resembles chabasite.   Its specific gravity is between 3·2 and 3·4 and its hardness a little above that of carbonate of lime.   Before the blow-pipe fuses into a white glass which is coloured blue by nitrate of potash. It occurs in small crystals disseminated over the surface of a quartz rock.   Dufrenoy found its composition

Sulphate of barytes, . . .	61·731
Lime, ...	11·980
Silica, ...	9·712
Sulphuric acid, ...	8·346
Alumina, ...	2·404
Water, ...	2·308
Loss and Carbonic acid, .	3·519
	100·000

This is equivalent to $2 Br \dot{S}l + Cal \dot{S}l$; and hence may be considered as a purer form of the amorphous Calcareo-sulphate of barytes described by Dr. T. Thomson in this Journal, vol. i. 370.

IV.—*On the influence of the Moon on the Barometer.*

The Rev. Mr. Everest, of Bengal, has shewn, that on an average of 10 rainy seasons, in India, the daily amount of the rain-fall diminished as the declination of the moon increased, until it reached between 10° and 15°; but after that distance, the reverse took place, and the amount of rain-fall increased as the declination increased.   The general average of 10 years for every 5° distance from the equator, gave the following curve:

$$\begin{array}{ccccc} 0^\circ\, 5^\circ & 10^\circ & 15^\circ & 20^\circ & 25^\circ \\ \cdot321 & \cdot271 & \cdot256 & \cdot259 & \cdot347 \end{array} \Bigg\} \text{ from the Equator.}$$

He has also found that the greatest depressions of the barometer do not (as some have conjectured) coincide with the days of conjunction and opposition of the moon, neither with the days of her perigee; but that they coincide, or nearly so, with the days of her maximum monthly declination.—*Bengal Asiatic Soc. Journal, May,* 1835.

SUMMARY OF OBSERVATIONS OF THE BAROMETER, THERMOMETER, &c., for the year 1835.

(Made at the Manse of the Parish of Abbey St. Bathan's, Berwickshire, Lat. 55° 52′ N. Long. 2° 23′ W. at the height of about 450 feet above the sea.) By the Rev. JOHN WALLACE.

1835. Months.	THERMOMETER. At X. A.M. P.M.	At X. P.M.	Means.	At IX. A.M.	At IX. P.M.	Means.	HYGROMETER. (Leslie's.) At X. A.M	At X. P.M	Means.	At IX. A.M.	At III. P.M.	Means.	BAROMETER. (At 32° Fahrenheit.) At X. A.M.	At X. P.M.	Means.	At IX. A.M.	At III. P.M.	Means.	Temperature of Spring Water.	Mean point of Deposition.	Grains of Moisture in a cubic inch of air.	Relative Humidity.	Rain in inches.
January	36·1	34·2	35·1	34·8	37·3	36·0	10	10	10	9	11	10	29·460	29·468	29·464	29·455	29·449	29·452	—	27·2	·00116	780	1·077
February	40·7	36·8	38·7	39·2	41·6	40·4	9	8	9	7	12	9	29·045	29·033	29·039	29·041	29·039	29·040	—	32·2	·00138	836	2·692
March	43·3	36·8	40·0	41·7	44·3	43·0	11	5	8	8	15	12	29·332	29·396	29·364	29·327	29·351	29·339	41·0	34·2	·00147	845	2·642
April	47·5	41·5	44·5	46·1	47·9	47·0	23	12	17	21	25	23	29·582	29·580	29·581	29·573	29·560	29·566	42·2	33·7	·00145	714	0·910
May	49·7	43·0	46·3	48·8	51·0	49·9	25	9	17	23	27	25	29·310	29·317	29·313	29·316	29·302	29·309	46·3	37·3	·00159	743	2·658
June	56·4	49·5	52·9	55·0	58·5	56·7	28	10	19	27	30	28	29·524	29·533	29·528	29·524	29·511	29·518	45·7	45·3	·00208	782	2·018
July	58·9	51·8	55·3	57·1	61·5	59·3	29	10	20	29	33	31	29·444	29·450	29·447	29·444	29·434	29·439	46·3	47·0	·00223	777	1·162
August	60·7	54·6	57·7	59·0	63·2	61·1	24	8	16	19	29	24	29·438	29·454	29·446	29·437	29·422	29·430	47·0	51·4	·00258	835	3·478
September	53·4	48·0	50·7	51·5	56·3	53·9	16	4	10	13	21	17	29·091	29·053	29·072	29·098	29·058	29·078	46·3	45·5	·00213	873	4·033
October	45·7	42·1	43·9	43·9	47·3	55·6	11	7	9	8	16	12	28·513	28·551	28·532	28·511	28·522	28·517	46·0	39·0	·00165	855	4·992
November	41·7	40·3	41·0	40·8	41·8	41·3	9	5	7	8	8	8	29·362	29·319	29·341	29·365	29·337	29·351	45·7	36·9	·00157	872	4·488
December	37·0	36·6	36·8	36·4	37·9	37·1	8	6	7	7	9	8	29·572	29·585	29·578	29·569	29·561	29·565	45·7	32·0	·00135	906	2·413
Means.	47·6	42·9	45·2	46·2	49·1	47·6	17	8	12	15	19	17	29·306	29·312	29·309	29·305	29·295	29·300	45·2	38·5	·00135	818	Amount for the year 32·563

It may be proper to accompany the preceding Summary with a few remarks.

1. To determine the average or mean temperature of the atmosphere, at any given place, is a leading problem in Meteorology. For the solution of this problem various methods have been proposed ; but among these none seems more simple and convenient than that proposed by the author of the article *Meteorology,* in the Edinburgh Enclopædia ; namely, to take the average of the temperatures at 10 o'clock, morning and evening, for the mean temperature of the day. The fact of the mean of the temperatures at these two hours, corresponding very nearly with the daily mean, has been ascertained and confirmed by accurate observation. To render the observations, made during the past year, at this place, subservient to the purpose of determining the mean temperature, I accordingly adopted this principle, recommended both by simplicity, and by the accuracy of the results to which it leads, and noted regularly, the height of the thermometer at 10 A.M. and at 10 P.M. The third column in the preceding tabular summary exhibits the mean for each month ; and the average of these means gives 45°·2, for the mean temperature for the year.

In order to have a check upon the result, I examined about the middle of each month, the temperature of a copious well shaded spring issuing from the northern side of a hill. The thermometer employed for this purpose, I find to stand about three-fourths of a degree higher than the thermometer with which the temperature of the atmosphere is observed. In the monthly report no allowance was made for this difference ; but this has been done in the preceding summary ; and the average temperature of spring water for the year is found to be 45°·2 ; a result corresponding precisely with that obtained for the mean temperature of the atmosphere.

2. As the relative humidity of the atmosphere depends in no small degree on temperature, it may reasonably be concluded, that since the mean temperature, at 10 o'clock morning and evening, does not differ materially from the mean temperature for the whole day, the average of the hygrometer at these hours will be an approximation to the mean of the day. It is on this principle that I have registered the hygrometer at 10 A.M. and 10 P.M., and have recorded the results thus obtained as the means for the several months. Taking the average for the whole year, we obtain for the mean height of Leslie's hygrometer, at this place, about 12°.

3. With regard to the barometer, I am not aware of any principle that should lead to the choice of any other hours of observation, than those adopted for the thermometer and hygrometer. I have, accordingly, noted the height of the barometer also, at 10 A.M. and 10 P.M., and considered the mean of these observations as the mean for the whole day. The mean thus obtained for the whole year is 29·309 inches.

4. It seems to be worth while to compare this last result with the mean height of the barometer on the sea shore, and to determine thence the height of the place of observations. Now, according to the most accurate observations, the average height of the barometer for our climate, at the level of the sea, appears to be 29·830 inches.

We obtain, therefore, Log. 29·830 = 1·47465
                       Log. 29·309 = 1·46700

(*Note*—.If we take the more common estimate, 29·82, for ———
the mean barometric height, we then have Log. 29·82= ·00765
1·4750 — Log. 29·309=1·46700=·00750=75 fathoms or   10000
450 feet, the exact number given monthly by Mr. Wallace. ———————
Edit.)   76·5 fathoms.

Hence, it appears that the height of this place, above the sea, is somewhere about 76 fathoms, or 456 feet. This result agrees very well with what has been given in the monthly report as about the height of the Manse above the sea. As the temperature is supposed to be 32°, no correction, of course, is required on account of temperature.

5. The observations recorded at 9 o'clock A.M. and 3 o'clock P.M. were taken with a view of being compared with the corresponding results obtained at the apartments of the Royal Society, London, where 9 A.M. and 3 P.M. are the hours of observation.

6. The number of grains of moisture in a cubic inch of air has been deduced from the following formula given by the author of the article *Hygrometry* in the Edinburgh Encyclopædia.

Let t denote the temperature, B the height of the barometer, L the height of Leslie's hygrometer, and g the number of grains in a cubic inch of air under these circumstances; also, let f_t denote the elastic force of vapour at the temperature t. Then,

$$g = \frac{\cdot10953 \left(f_t - \frac{\frac{1}{6} \text{B L}}{1000 - \frac{1}{2}\text{L}} \right) \text{B}}{447\cdot4 + t}.$$

7. In order to obtain a numerical expression of the relative humidity of the atmosphere, let 1000 be assumed to denote absolute moisture, and 0 to denote absolute dryness. The numbers denoting the intermediate states of humidity are found by means of the above formula; for we have only to determine how many thousandth parts the *actual* quantity of moisture in the atmosphere is of the *whole* moisture, which it is capable of holding in solution at the given temperature. Thus, to determine the relative humidity for the medium state of the atmosphere in the month of May: put in the above formula $t = 46\cdot3$, B = 29·313, and L = 0, the value corresponding to complete saturation; we obtain $g = \cdot00214$ grs. But the actual moisture, at the mean height of the hygrometer for May, (namely 17°), is ·00159 grs.; and ·00214 : ·00159 :: 1000 : 743. Therefore, we have, for the relative humidity of the atmosphere, corresponding to the mean height of the thermometer, barometer, and hygrometer in May, the numerical expression 743; which simply means, that the actual moisture, corresponding to the medium state of the atmosphere in May, is $\frac{743}{1000}$th parts of the whole moisture, which, under these circumstances, it can hold in solution.

8. I will only remark farther, that the mean point of deposition for the year is 6°·7 below the mean temperature;—a result which also agrees with what was to be expected.

Without laying any undue stress on the coincidences here pointed out, as indicating accuracy in the observations from which the results have been deduced, I may be allowed to remark, that there has been no adjusting of numbers for the purpose of bringing out results *apparently* satisfactory.

Meteorological Journal,

t at the Manse of the Parish of Abbey St. Bathan's, Berwickshire, Lat. 55° 52' N. Long. 2° 23' W. at the height of about 450 feet above the sea. By the Rev. John Wallace.

JANUARY.

DATE. 1856.	THERMOMETER. IX A.M.	X A.M.	III P.M.	X P.M.	HYGROMETER (Leslie's.) IX AM	X AM PM	X PM	BAROMETER. At 32° of Fahrenheit. IX A.M.	X P.M.	X P.M.	Rain in Inches Weekly.	Direction of Wind at X A.M.	REMARKS.
Fr. 1	32¼	32¾	32	29	3	4	7	29·686	29·695	29·920		S.E. by E.	Brisk wind, sky overcast, occasionally showers of snow.
Sa. 2	32¼	34¼	35½	35¼	4	10	5	33·014	30·014	29·947		W.N.W.	Calm, overcast and lowering, in the evg. the clouds in rapid motion from S.W.
Full M. Mo. 3	38¼	40½	41	46½	5	9	12	29·748	29·722	29·428		NW by W	Calm interrupted by sudden gusts, sky overcast and often lowering.
Tu. 4	44¼	43½	48½	46	7	7	8	29·431	29·397	29·475	0·363	W. by N.	Calm, drizzling rain, evg. fair, clouds attaining a cirrocumulative appearance.
We. 5	42	42½	46	46	0	0	10	29·560	29·565	29·555		S. S. E.	A.M. calm, with mist and drizzling rain, P.M. cloudy, frequent gusts of wind.
Th. 6	41½	40½	42½	41	8	8	6	29·594	29·590	29·530		E. S. E.	Calm, cloudy, and occasionally lowering.
Fr. 7	38	38½	40	33½	5	3	9	29·400	29·426	29·289		S. E.	Gentle breeze, soft hazy clouds abundant.
Sa. 8	35½	38½	38	35½	3	6	8	29·417	29·433	29·487		S. E.	Wind gentle and brisk alternately, large masses of clouds prevalent, evg. calm.
Su. 9	35½	35½	35½	35	2	4	0	29·347	29·346	29·493		E. S. E.	Wind, hazy clouds overspreading the sky, in the evening a slight shower.
Mo. 10	32½	34½	32½	31	12	13	8	29·362	29·356	29·236		E. by N.	A.M. gentle breeze, P.M. wind strong and gusty, hail showers in the evening.
Tu. 11	28	31½	33	31	3	5	6	28·787	28·789	28·762	0·735	N. W.	Calm, sky overcast, frequent snow-showers, in the evening gusts of wind.
We. 12	33½	34	36½	37	10	6	3	28·849	28·862	28·900		S. W.	Brisk wind, A.M. cloudy (cirri and cirrostr.) snow-shower, clear & frosty.
Th. 13	39	39½	42½	35	9	2	14	29·019	29·010	28·953		S. W.	Brisk wind, sky veiled with thin cirrostratus, soft clouds floating below.
Fr. 14	30½	32½	35	35	6	2	6	28·898	28·862	28·684		NW by W	Strong wind, clouds passing in masses under higher cirrostr. showers, evg. wind N.
Sa. 15	28½	30½	33	34½	4	4	10	28·688	28·713	28·797		NW by W	A.M. brisk wind, P.M. wind strong, heavy black masses of cloud, snow showers.
Su. 16	32½	34½	35½	38½	8	5	10	29·611	29·632	29·587		NW by W	A.M. cirri abundant and polarized from N. E. to S. W.
Mo. 17	41½	44½	47½	32	9	7	12	29·681	29·692	29·703		W.	Gentle breeze, cirrostratus tending to cirrocumulus, evg. cloudy, wind rising.
New M Mo. 18	28½	29	29	29	9	11	6	29·316	29·287	29·506		W. by S.	Strong wind, sky overcast, P.M. wind veering N., snow-showers.
We. 19	34½	35½	38½	36	5	12	9	29·812	29·817	29·709		S.	Calm, A.M. black masses of cloud which gradually dissipated, evg. overcast.
Th. 20	37½	39	41	40½	5	7	6	29·556	29·552	29·446	0·500	S.	Calm, cirrostratus prevalent, evening clear with tendency to frost.
Fr. 21	40½	41	40½	37½	6	2	10	29·078	29·065	28·949		S. W.	Brisk wind, cirrostratus prevalent, evening overcast.
Sa. 22	41	41½	41½	36½	6	4	7	28·148	28·161	28·257		S. W.	Calm, morning showery, light clouds below patches of cirrostr, evg. cloudy.
Su. 23	34	35½	42	43	4	5	5	29·277	29·327	29·640		S. W.	Strong and boisterous wind with driving rain.
Mo. 24	42½	42½	43½	35½	12	9	4	29·656	29·660	29·537	1·313	S. W.	Very calm, overcast, sometimes lowering, soft showers, evg. partially clear.
Tu. 25	33½	33½	36½	39	9	10	5	29·364	29·382	29·248		S. W.	Calm, with hoar frost in the morning, cloudy after 10 A.M. brisk wind.
We. 26	33	33	36½	32	9	5	17	29·204	29·216	28·743		S. W.	Very brisk wind, cloudy, evening calm with rain, wind veering N.
Th. 27	35	36½	36½	27	10	13	6	28·797	28·805	28·609		W. by S.	Strong wind, sky overcast, even: g showery, wind increasing and veering N.
Fr. 28	31½	31½	30½	30	9	9	5	28·570	28·550	28·424		W.N.W.	Brisk wind, sky cloudy and stormy aspect, evening calm and nearly cloudless.
Sa. 29	36	36	34½	34½	18	18	11	28·396	28·462	28·734	0·375	N.W.W.	Brisk wind sinking occasionally into calm, unsettled sky with snow-showers.
Su. 30	33	34	36½	34½	2	2	5	28·486	28·426	28·389		SW by W	Strong wind, sky overspread with a hazy whiteness.
													Wind rising in sudd: n gusts followed with calms, unsettled sky, snow-showers.
Means.	36	36·7	38	35·9	6	8	7	29·219	29·218	29·196	3·086		Means { Therm. 36·3 for the { Hygr. 7. Month { Bar. 29·207 in. } Mean temperature of spring water 45°5 } Mean point of deposition 31°·5 { Moisture in a cubic inch of air = ·001309 grs.

RECORDS

GENERAL SCIENCE.

ARTICLE I.

Memoir of Dr. Thomas Young. By M. ARAGO.*

THOMAS YOUNG was born at Milverton, in Somersetshire,
on the 13th 'of June, 1773, of parents who belonged to the
sect of Quakers. He passed the first years of his life at
the house of his maternal grandfather, Mr. Robert Davies
of Minehead, whom active commercial engagements had
not prevented from cultivating the classics. Young could
read fluently at the age of *two years.* His memory was
quite extraordinary. During the intervals from his long
engagements at a school kept by a mistress in the neigh-
bouring village of Minehead, he had learned by heart, at
four years, a great number of English authors, and even
different Latin poems, which he could recite from begin-
ning to end, although he did not understand this language.
The name of Young, like many other celebrated names
already collected by biographers, will contribute then to
support the hopes or fears of many good fathers, who see
in some well recited lessons, without fault or mistake, from
the latter, the sure proof of eternal mediocrity; from the
former, the infallible entrance upon a glorious career. We
should have departed strangely from our object if these
historic notices could strengthen such prejudices. There-
fore, without a desire of weakening the lively and pure
emotions which are annually excited by the distribution of
prizes, we might remind some in order that they may
not give way to dreams never to be realized, and others,

* From the Revue des Deux Mondes, December, 1835.

to prevent them from being discouraged, that Pic de la Mirandole, the phenix of the scholars of every age and country, was, at a mature age, an insignificant author; that Newton, the powerful intelligence of whom Voltaire has said, without giving way to any exaggeration,

> " Confidens de Très-Haut, substances éternelles,
>   Qui parez de vos feux, qui couvrez de vos ailes
>   Le trône où votre maître est assis parmi vous,
>   Parlez, du grand Newton n'étiez-vous point jaloux ? "

that the great Newton, we say, made little progress in the classics during his terms at college; that study had at first no attractions for him; that the first time he perceived the necessity of labouring, was in order to get above a turbulent student, who, seated, on account of his rank, on a higher bench than himself, tormented him by kicking him; that in his 22d year he competed for a Fellowship at Cambridge, and was beat by a certain Robert Uvedale, whose name, without this circumstance would be entirely forgot; that Fontenelle applied with less accuracy than ingenuity these words of Lucan, " Men have not been permitted to see the Nile feeble at its source."

At the age of six, Young was given to the care of a teacher, at Bristol, whose mediocrity proved fortunate for him. This is not paradoxical; the pupil, unable to submit to the slow and circuitous path of the master, became his own instructor; and thus, those brilliant qualities were developed which too much assistance would certainly have weakened.

Young was eight years old when chance, whose influence in the events of the lives of all men, is more considerable than their vanity considers it prudent for them to admit, relieved him from his exclusively literary studies, and pointed out his proper vocation.

A land surveyor of great merit, near whom he dwelt, took a great liking to him. He carried him away sometimes over the land during the holydays, and allowed him to enjoy, with his instruments geodesy and physics. The operations by which the young scholar saw how to determine distances, and the elevation of inaccesible objects struck his imagination; but speedily some chapters of a mathematical dictionary dissipated every thing which appeared mysterious. After this time, in the Sunday pro-

menades, the quadrant re-placed the paper kite. In the evening, by way of relaxation, the engineer apprentice calculated the heights measured in the morning.

From the age of nine to his 14th year, Young lived at Compton, in Dorsetshire, with a Mr. Thomson, whose memory was always dear to him. During these five years all the boarders were exclusively occupied, as is the custom in English schools, with a minute study of the Greek and Latin writers. Young was always at the head of his class; and, yet at the same time he learned French, Italian, Hebrew, Persian and Arabic; the French and Italian he learned in order to satisfy the curiosity of a companion who had in his possession several works printed at Paris, of which he wished to know the contents; the Hebrew, to read the Bible in the original; the Persian and Arabic with a view of deciding this question raised during a conversation at dinner, Are the differences between the Oriental languages as marked as between those of Europe?

I feel the necessity of observing, that I write from authentic documents, before adding, that during the time he made such extraordinary progress in the languages, he acquired, in his walks around Compton, a lively passion for botany; that, deprived of the means of magnifying, which naturalists make use of, when they wish to examine the most delicate parts of plants, he undertook himself to construct a microscope without any other guide than a description of this instrument given by Benjamin Martin; that in order to arrive at this difficult result, it was necessary to acquire, previously, much dexterity in the art of turning; that the algebraical formulæ of the optician presenting symbols of which he had no idea (symbols of *fluxions*), he was much perplexed for a while; but, that unwilling to renounce his desire of magnifying the pistils and stamens, he found it more simple to learn the differential calculus in order to understand the unfortunate formulæ, than to send to the neighbouring town to purchase a microscope.

The brilliant activity of Young made him pass the bounds of human strength. At 14 years, his health was greatly changed. Different symptoms threatened disease of the lungs, but these threatening symptoms yielded to the prescriptions of the art, and to the great care his parents took of him.

It is common with our transmarine neighbours, for a rich

person, in trusting his son to a particular preceptor, to look for a companion in study among the young persons of the same age who have already distinguished themselves by their success.   Young assumed this title in 1787, and became fellow - pupil of the grandson of Mr. David Barclay of Youngsbury, in Hertfordshire.   On the day of his installation, Mr. Barclay gave him some sentences to copy in order to ascertain if he wrote a good hand.   Young, perhaps, feeling humbled by this kind of proof, for the purpose of giving satisfaction, requested permission to retire into the adjoining room.   His absence having been prolonged beyond the time that the mere transcribing of the copy required, Mr. Barclay began to joke about the young quaker's want of dexterity, when at last he entered.   The copy was remarkably beautiful; a writing master could not have performed it better.   No remark could be made on the time of his absence, for the young quaker, as Mr. Barclay called him, was not content with transcribing the English sentences given him, he had translated them into nine different languages.

The preceptor, or as he is called on the other side of the channel, the *tutor,* who directed the studies of the two scholars of Youngsbury, was a distinguished young man then engaged in perfecting himself in a knowledge of the ancient languages, and was afterwards author of the *Calligraphia Græca.*   He was not long, however, in perceiving the great superiority of one of the two pupils, and observed with the most laudable modesty, that in their common studies, the true *tutor* was not always he who bore the title.

At this period Young drew up from original sources, a detailed analysis of the numerous systems of philosophy which were professed in the different schools of Greece. His friends speak of this work in very high terms.   I am not aware if the public are ever to have it presented to them.   It had a decided effect upon the life of its author, for, in making an attentive and minute examination of the fantasies (bizarreries), (I use a polished word) with which the conceptions of the Greek philosophers abound, Young felt the attachment weaken which he had hitherto preserved for the principles of the sect in which he was born. He did not, however, separate himself entirely from it until

some years afterwards, during his residence in Edinburgh. The little colony of students, at Youngsbury, left Hertfordshire for some months in winter, and went to live in London. During one of these excursions, Young met a professor worthy of him. He was initiated into chemistry by Dr. Higgins, of whom I am more disposed to make mention, since, notwithstanding his repeated and urgent claims, some have not recognized the part which legitimately belongs to him in the theory of definite proportions,* one of the finest acquisitions of modern chemistry.

Dr. Brocklesby, the maternal uncle of Young and one of the principal physicians in London, justly proud of the great success of the young scholar, communicated some of his compositions to philosophers, to literary men, and to men of the world, whose approbation could most flatter his vanity. Young became thus, at a very early period, personally acquainted with the celebrated Burke and Windham of the House of Commons, and with the Duke of Richmond. The latter, then Commander-in-chief, offered him the situation of Assistant-secretary. The other two statesmen, although they were anxious for him to follow a political career, recommended him to go, in the first place, to Cambridge to study a course of law. With so many powerful patrons Young could have calculated on the possession of one of those lucrative situations which persons in power are anxious to bestow upon such as will do the duties with steady application, and furnish them with the means daily of shining at court or council, without compromising their vanity by any indiscretion. Young fortunately was conscious of his powers; he felt within him the germ of those brilliant discoveries which have since rendered his name illustrious; he preferred the laborious though independent career of a literary life to the golden chains which shone before his eyes. Let this be said to his honour! Let his example serve as a lesson to many young people who are authoritatively turned aside from their noble vocation to be converted into *bureaucrates.* Like Young with their eyes directed to the future; let them not sacrifice to the futile and fleeting satisfaction of being surrounded with petitioners, wit-

* Was this not Dr. Higgins of London, the claimant of the discoveries of Priestley, and father of Dr. Higgins of Dublin, the claimant of the Atomic theory? —EDIT.

nesses of esteem and gratitude, which the public rarely fails to pay to intellectual labours of a high order. And, if it should happen, that in the illusions of inexperience, they should find too dull a sacrifice prescribed to them, we would request them to receive a lesson of ambition from the great captain, whose ambition knew no bounds, to meditate on these words which the first consul, the victor of Marengo, addressed to one of our most honourable colleagues (M. Lemercier), when the latter refused the very important situation at that time, of state counsellor. " I understand, Sir, you are fond of learning, and you wish to devote yourself entirely to it. I have no opposition to offer to this resolution. Yes! Do you think that if I had not become commander-in-chief, and the instrument of the destiny of a great people, I should have passed through offices and halls to be dependent on a person in power, in quality of minister or ambassador? No, No! I should have entered on the study of the exact sciences. I should have traversed the path of Galileo and Newton; and since I have constantly succeeded in my great enterprizes, Ah, well! I should have been highly distinguished also by my scientific works; I should have left the remembrance of fine discoveries. No other glory would have invited my ambition."

Young made choice of the career of medicine, in which he hoped to find fortune and independence. His medical studies began at London under Baillie and Cruickshank. He continued them at Edinburgh, where then Drs. Black, Munro and Gregory were highly distinguished; but it was only at Goëttingen in the following year (1795) that he took his degree.

Before submitting to this vain formality, yet so decidedly required, Young had scarcely exceeded his boyhood; he had already distinguished himself before the scientific world by an observation relating to gum *Ladanum;* by a controversy which he had carried on with Dr. Beddoes on Crawford's Theory of heat; by a memoir concerning the habits of spiders, and the system of Fabricius, the whole enriched with erudite researches; and lastly, by a work which I shall notice at greater length on account of its great merit, of the unusual favour which it produced, and of the oblivion into which it has since fallen.

The Royal Society of London enjoys over all the three kingdoms a high and deserved character. The *Philosophical Transactions* which it has published for a century and a half, the glorious archives where British genius considers it an honour to deposit its titles to the gratitude of posterity. The desire of seeing their names inserted in lists of contributors to this truly national collection, after the names of Newton, Bradley, Priestley and Cavendish, has always been the most active, as it is the most legitimate subject of emulation among the students of the celebrated universities of Cambridge, Oxford, Edinburgh and Dublin. That is the boundary of the ambition of the man of science; he only aspires to it on account of some capital work, and the first essays of youth are communicated to the public in a mode better fitted to their importance, through the medium of those numerous *Reviews* which with our neighbours have contributed so much to the progress of human knowledge. Such, consequently, ought not to be the course of Young. At the age of twenty, he addressed a memoir to the Royal Society. The council, composed of all the contemporary men of note, honoured this paper with its vote, and it soon appeared in the *Transactions*. The subject of it was vision. The problem was nothing less than new. Plato and his disciples four centuries before our era had been occupied with it, but their conceptions could only be cited to justify the celebrated and humbling remark of Cicero : " Nothing can be imagined so absurd which will not be supported by some philosopher."

After an interval of twenty centuries, it is necessary to proceed from Greece to Italy, when we wish to find in reference to the admirable phenomenon of vision, opinions which deserve to be noticed by the historian. There without interdicting, like the philosopher of Egina, their stay, to all those who were not geometricians, prudent experimenters, followed the only road by which man can be permitted to arrive without a false step at the conquest of things unknown; there Maurolycus and Porta proclaimed to their contemporaries, that the problem of discovering *that which is,* presents a sufficient number of difficulties without presuming to discover *what ought to be;* there these two celebrated countrymen of Archimedes began to develope the catalogue of the different media of which the eye is

composed, and shewed themselves satisfied, as Galileo and
Newton subsequently were, not to raise themselves above
the knowledge susceptible of being controlled by the senses,
and which was stigmatized, under the porches of the Aca-
demy, by the disdainful qualification of *simple opinion.*
Such is always human weakness, that after having followed
with wonderful success, the principal inflexions of light
through the cornea and crystalline lens, Maurolycus and
Porta, having nearly attained their object, stopped short,
suddenly, as before an insurmountable difficulty which
opposed their theory, viz., that objects ought to appear up-
side down if the images in the eye are themselves reversed.
The persevering mind of Kepler, on the contrary, pre-
vented him from giving way. The attack is psycological.
The objection is overturned by psycological clearness and
mathematical precision. Under the powerful influence of
this great man, the eye becomes distinctly the simple op-
tical apparatus known under the name of obscure chamber,
the retina is the picturé, the crystalline lens compensates
for the vitreous humour.

This assimilation, so generally adopted after Kepler, gave
rise to one difficulty only. The obscure chamber like a com-
mon glass *must be placed at a focus* according to the distance
of objects. When the objects approach, it is indispensible
to evert the picture by the lens; a contrary movement is
necessary when the objects are distant. To preserve to the
images all desirable precision, without changing the posi-
tion of the surface which receives them, is then impossible,
at least, that the curvature of the lens may always vary.

Among the different modes of obtaining distinct images
nature has afforded a choice; for man can see with great
precision, at very dissimilar distances. The question thus
stated, has afforded a subject of great research and dis-
cussion to philosophers. Great names figure in the debate.

Kepler, Descartes, ... support the opinion, that the
whole of the globe of the eye is susceptible of being elon-
gated and flattened.

Porterfield, Zinn, ... would have the crystalline
lens to be moveable for the purpose of placing itself at a
greater or less distance from the retina.

Jurin, Musschenbroek, ... believe in a change in the
curvature of the cornea.

Sauvages, Bourdelot, ... consider also, that a variation in the curvature occurs, but only in the crystalline lens.

Such is also the system of Young. Two memoirs presented successively to the Royal Society contain its complete development.

In the first, the question is only viewed in an anatomical light. Young demonstrated, by means of direct and very delicate observations, that the crystalline lens is possessed of a fibrous or muscular structure, admirably adapted to all kinds of changes in form. This discovery overturned the only solid objection which had hitherto been opposed to the hypotheses of Sauvages, Bourdelot, &c. Scarcely was it published when Hunter claimed it. The celebrated anatomist thus assisted the interest of the young beginner, since his work, unpublished, had been communicated to no one. However, this part of the discussion soon lost all its importance, for it was shewn that Leeuwenhoek, supplied with powerful microscopes, had already traced and designed the muscular fibres of the crytsalline lens of a fish in all their ramifications. To awaken the public attention, fatigued with so many debates, nothing less would have satisfied than the high renown of two new members of the Royal Society who entered the lists. One of these an expert anatomist, the other the most celebrated artist of which England could then boast, presented to the Royal Society a memoir, the fruit of their combined efforts, and destined to establish the completely unalterable nature of the crystalline lens. The philosophical world, with the greatest hesitation, would have admitted, that Sir Everard Home and Ramsden together could have made inaccurate experiments; that they could have been deceived in the micrometical measurements. Young himself did not believe it; and immediately in a public manner renounced his theory. This eagerness to confess that he was overcome, so rare in a young man of twenty-five years, so rare especially in the case of a first publication, was here an act of unexampled modesty. Young, in fact, had nothing to retract.

In 1800, having withdrawn his disavowal, he developed anew the theory of the change in form of the crystalline lens in a memoir, to which no serious objection has been since made. Nothing can be more simple than his line of argument, nothing more ingenious than his experiments.

Young examines first the hypothesis of a variation in the curvature of the cornea, by microscopical observations, which would have rendered the smallest variations appreciable; or we may say more distinctly, he places the eye in certain conditions where the changes in the curvature should be without any effect, he plunges it into water and proves, even then, that the faculty of seeing at different distances, remains in it complete.

The second of the three possible suppositions, that of an alteration in the dimensions of the organ, is then destroyed by a number of objections and experiments which it would be difficult to overturn.

The problem appeared definitely settled. For it is obvious, that if, of three possible solutions, two are removed, the third necessarily follows, that the radius of curvature of the cornea and the longitudinal diameter of the eye being unalterable, the form of the crystalline lens must vary. Young, however, did not stop here, he proved directly, by the subtile phenomena of the change in form of the images, that the lens really changes its curvature; he invented, or at least, improved an instrument susceptible of being employed, even by persons of little intelligence, and little accustomed to delicate experiments; and provided with this new method of investigation, he renders it certain that all those who want the crystalline lens, as in consequence of the operation for cataract, do not enjoy the faculty of seeing precisely at different distances.

It is truly matter of astonishment, that this admirable theory of vision, this fine wove network, where reasoning and the most ingenious experiments afford mutual support, does not occupy the important place in science which it ought. But to explain this anomaly, we must necessarily recur to a sort of fatality. Young, had he been, as he often said with regret, a new Cassander, incessantly proclaiming important truths, his ungrateful contemporaries would have refused to receive them. We should be speaking, it appears to me, less poetically, and with more truth, in remarking, that the discoveries of Young are unknown to the most of those who should be best able to appreciate them. Physiologists do not read his excellent memoir, for he takes for granted the acquisition of more mathematical knowledge than is usually cultivated by the faculty.

Natural philosophers in their turn have also overlooked it, because in their lectures and works, the public of the present day only require such surperficial notions as can be readily grasped by a common mind without any exertion. In all this we see nothing exceptional ; like all those who penetrate into the depth of science, he has been unknown to the multitude. But the applause of some men of eminence should make ample compensation. In such matters we ought not to count votes, it is wiser to weigh them.

(*To be continued.*)

ARTICLE II.

Observations on the Atomic Weights of Bodies. By THOMAS THOMSON, M. D., F. R. S. L. & E., Regius Professor of Chemistry in the University of Glasgow.

(*Concluded from* page 193.)

IT seems to be a general law, that the specific gravity of a gas is equal to its atomic weight multiplied by some submultiple or multiple of the specific gravity of oxygen gas. This will appear from the following tables.

I. Gases whose specific gravity = their atomic weight × 0·2777 or ¼th the specific gravity of oxygen gas.

	Atomic weight.	Specific Gravity, By experiment.	Specific Gravity, By calculation.
Ammonia, ...	2·125	0·59023*	0.59027
Deutoxide of azote, .	3·75	1·04096†	1·04166
Muriatic acid, . . .	4·625	1·2844‡	1·28472
Hydriodic acid, . . .	15·825	4·4093§	4·40972
Hydrobromic acid, . .	10·125	——	2·8125
Calomel, ...	29·5	8·35‖	8·1944

* This is the mean between the specific gravity found by Sir H. Davy and myself.—See Annals of Philosophy, xvi. 175.

† By my experiment.—Ibid. p. 172.

‡ By my experiment.—Ibid. p. 176.

§ Mean of the determination of Gay Lussac and my own.—Ibid. p. 264.

‖ Mitscherlich.—Poggendorff's Annalen, xxv. 223.

In all these gases the calculated specific gravity comes within less than $\frac{1}{1000}$th part of the experimental, except in the case of calomel, which, being a vapour, and requiring a high temperature to keep it in the gaseous state, cannot be expected to be determined with minute accuracy.

II. Gases whose specific gravity = their atomic weight × 0·5555 or $\frac{1}{2}$ the specific gravity of oxygen gas.

	Atomic weight.	Specific Gravity,	
		By experiment.	By calculation.
Hydrogen, ...	0·125	0·0694*	0·0694
Azote, ...	1·75	0·9728*	0·9722
Carbon vapour, ...	0·75	——	0·4166*
Chlorine, ...	4·5	2·5†	2·5
Bromine, ...	10	5·54‡	5·5555
Iodine, ...	15·75	8·716§	8·7166
Mercury, ...	12·5	7·03‡	6·9444
Carburetted hydrogen, . .	1	0·5554†	0·5555
Olefiant gas, ...	1·75	0·9709†	0·9722
Carbonic acid, ...	2·75	1·5267*	1·5277
Carbonic oxide, ...	1·75	0·9698†	0·9722
Chloro-carbonic acid, . . .	6·25	3·4604†	3·4722
Protoxide of azote, ...	2·75	1·5269†	1·5277
Cyanogen, ...	3·25	1·8039†	1·8055
Sulphurous acid, ...	4	2·22216*	2·2222
Sulphuric acid (dry), . . .	5	3·00‡	2·7777
Selenious acid, ...	7	4·03‡	3·8888
Iodide of mercury	28·25	16·2‡	15·6944
Corrosive sublimate, . . .	17	9·8‡	9·4444
Sesquiodide of arsenic, . . .	28·375	16·1‡	15·7638
Proto-chloride of Antimony, .	12·5	7·8‡	6·9444

In all the gaseous compounds of carbon, hydrogen, oxygen and azote, and also in chlorine and bromine, and iodine vapour, the specific gravities determined by multiplying the atomic weight by 0·5555 almost coincide with the ex-

* These specific gravities have been given in a preceding part of this paper.

† These specific gravities were determined by me in 1820.—See Annals of Philosophy, xvi. 161 and 241.

‡ Dumas Ann. de Chim. et de Phys., xxxiii. 346.

§ Mitscherlich.—Poggendorff's Annalen, xxix. 217.

perimental result, rendering it exceeding probable, that the number which we have pitched upon for the atomic weight of these bodies is indeed very near the truth. In the specific gravities of the vapours determined by Mitscher- lich, the coincidence between the calculated and experi- mental results is not so close. Because, from the high temperature at which these specific gravities were taken, sometimes as much as 643°; it was impossible to attain the same accuracy, as when the specific gravity is taken at the common temperature of the atmosphere. Nor, is it quite clear, that the corrections for the temperature which Mits- cherlich applied are perfectly correct.

III. Gases whose sp. gr. = atomic weight × 1·1111.

	Atomic weight.	Specific Gravity,	
		By ex- periment.	By cal- culation.
Oxygen, ...	1	1·1117	1·1111

IV. Gases whose sp. gr. = atomic weight × 2·2222, or twice the specific gravity of oxygen gas.

	Atomic weight.	Specific Gravity,	
		By ex- periment.	By cal- culation.
Phosphorus, . . .	2	4·6*	4·4444
Arsenious acid, . .	6·25	13·85*	13·8888

V. Gases whose specific gravity = atomic weight × 2·5, or 2¼ times the specific gravity of oxygen gas.

	Atomic weight.	Specific Gravity,	
		By ex- periment.	By cal- culation.
Arsenic, . . .	4·25	10·6†	10·625

VI. Gases who sp. gr. = atomic weight × 3·3333, or 3 times the specific gravity of oxygen gas.

	Atomic weight.	Specific Gravity,	
		By ex- periment.	By cal- culation.
Sulphur, . . .	2	6·9‡	6·6666

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* These specific gravities were determined by Mitscherlich.—See Poggendorff's Annalen, xxix. 218 and 222.

† Mitscherlich.—Ibid, p. 219.　　　　　‡ Ibid. p. 217.

A bare inspection of these tables shews how very close approximations to the truth my atomic numbers would be. This would be still further corroborated if we were to draw up tables exhibiting the constitution of the compound gases in volumes, and the degree of contraction which has taken place. But this paper has already run out to such a great length, and so much remains to be noticed, that I must delay these tables, at least, for the present.

There is another point, however, of so much consequence that it would be unpardonable to pass it by; because it furnishes a method of determining which of two numbers ought to be chosen for the atomic weight of a body, when we have arguments in favour of two different numbers almost equally balanced. Thus, we do not know for certain whether 0·125 or 0·0625 be the atom of hydrogen; or whether 1·75 or 0·875 be the atomic weight of azote. The point to which I allude is an observation of Dulong and Petit, that if the atomic weight of a body be multiplied into its specific heat, the product is a constant quantity. This subject has been lately taken up by M. Avogadro, who has made further researches into the specific heats of bodies, in order to put the law more completely to the test of experiment. He has found the law to hold good in most of the cases which have come under his review.

The reader will observe, that if the specific heat of bodies multiplied into the atomic weight be a constant quantity, it will follow, that every simple atom is surrounded with the same quantity of heat; or, in other words, that the specific heat of the atoms of all bodies is the same. This is so important a proposition that it deserves a rigid investigation.

Our methods of determining the specific heat of bodies are not yet so accurate that the numbers obtained can be relied on as perfectly correct. But as they constitute at least approximations to the truth, they will enable us to perceive whether or not the law of Dulong and Petit holds in general. Let us draw up tables of the atomic weights and specific heats of all the simple substances so far as known; and let us multiply these two numbers into each other, that we may see how far the product will be a constant quantity.

TABLE I.—Simple Substances.

	Atomic weight.	Specific heat.	Product.	
Carbon, ...	0·75	0·257*	0·19275	
Silicon, ...	1	0·1875?	0·1875	$=0·3\frac{7}{2}$
Aluminum, . . .	1·25	0·1500?	0·1875	
Oxygen, ...	1	0·2361	0·236	
Chlorine, . . .	4·5	0·0827	0·372	
Bromine, . . .	10	0·0472	0·437	
Hydrogen, . . .	0·125	3·2936	0·412	
Azote, ...	1·75	0·2698	0·472	
Sulphur, ...	2	0·188†	0·376	
Arsenic, ...	4·75	0·081*	0·385	
Antimony, . . .	8	0·047‡	0·376	
Tellurium, . . .	4	0·0912†	0·364	
Iron, ...	3·5	0·11†	0·385	Mean 0·375
Nickel,** . . .	3·625	0·1035†	0·375	
Zinc, ...	4·125	0·0927†	0·383	
Lead, ...	13	0·029†	0·377	
Tin, ...	7·25	0·051†	0·370	
Copper, ...	4	0·095†	0·376	
Bismuth, . . .	9	0·04§	0·360	
Mercury, . . .	12·5	0.029‖	0·3625	
Gold, ...	12·5	0·0298†	0·373	
Platinum, . . .	12	0·031†	0·372	
Cobalt, ...	3·25	0·15†	0·4875	
Phosphorus, . .	2	0·385*	0·770	
Silver, ...	13·75	0·056†	0·770	
Iodine, ...	15·75	·089 *	0·40175	

** I have adopted 3·625 for the atom of nickel partly from the experiments of Tuppuli and Rothoff, and partly because Berzelius's number is as high as 3·69675. But I have never been able, from my own experiments, to obtain so high an atomic number for nickel. I made a very careful analysis of sulphate of nickel very lately. The salt had been twice crystallized, and had all the characters of a pure and neutral salt. From 100 grains of it I got

Oxide of nickel,........25·17....4·41
Sulphuric acid,.........28·50....5
Water,................40·88
Impurity,............. 1·75
———
96·30

The water had it been all extracted would have amounted to a grain more. The loss is almost 3 per cent., and the 1·75 grain of impurity destroys the confidence I expected to have derived from this analysis.

TABLE II.—Binary Compounds.

	Atomic heat.	Specific heat.	Product.	
Silica, ...	2	0·179*	0·358	
Alumina, . . .	2·25	0·1942‡	0·437	
Protoxide of azote,	2·75	0·2369	0·651	
Lime, ...	3·5	0·179*	0·626	
Magnesia, . . .	2·5	0·276‡	0·690	Mean 0·788
Protoxide of lead, .	14	0·05*	0·700	0·375 × 2
Oxide of zinc, . .	5·125	0·137(c)	0·702	= 0·750
Oxide of copper, .	5	0·137‡	0·685	
Red oxide of mercury,	13·5	0·049*	0·665	
Protoxide of tin, .	8·25	0·096*	0·775	
Oxide of chromium,	5	0·196‡	0·980	
Sulphuret of lead,	15	0·053‡	0·795	
Sulphuret of mercury,	14·5	0·052‡	0·764	
Sulphuret of zinc,	6·125	0·153‡	0·701	
Sulphuret of iron,	5·5	0·135*	0·742	
Sulphide of arsenic,	6·75	0.1111‡	0·750	
Water, ...	1·125	1	1·125	Mean 1·150
Corrosive sublimate	17	0·069*	1·175	0·375 × 3
Common salt, . .	7·5	0·221*	1·657	= 1·125
Chloride of potassium,	9·5	0·184*	1·748	Mean 1·587
Chloride of calcium,	7	0·194*	1·358	0·375 × 4 = 1·500

TABLE III.—Compounds of 2½ atoms.

	Atomic weight.	Specific heat.	Product.	
Peroxide of iron, .	5	0·1691‡	0·846	Mean 0·847
Orpiment, . . .	7·75	0·105*	0·814	0·375 × 2½
Arsenious acid, .	6·25	0·141*	0·881	= 0·9375

TABLE IV.—Ternary Compounds.

	Atomic weight.	Specific heat.	Product.	
Carbonic oxide, .	3·5	0·2884	1·009	
Olefiant gas . .	1·75	0·4207	0·736	
Antimonious acid,	10	0·13‡	1·300	
Deutoxide of mang.,	5·5	0·191*	1·050	
Peroxide of tin, .	9·25	0·111*	1·027	Mean 1·071
Suboxide of copper,	9	0·1073‡	0·966	0·375 × 3 =
Titanic acid, . .	5·25	0·1724‡	0·905	1·125
Bi-sulphuret of iron,	7·5	0·135	1·212	
Bi-sulph. of molybden	10	0·102‡	1·021	
Calomel, ...	29·5	0·041*	1·209	
Brass, ...	12·25	0·11†	1·347	

TABLE V.—Quarternary Compounds.

	Atomic weight.	Specific heat.	Product.	
Sulphuric acid, . .	6·125	0·35§	2·144	0·375 × 6 = 2·25
Chrysolite, . . .	6·5	0·205‡	1·332	0·375 × 4 = 1·5

TABLE VI.—Quintenary Compounds.

	Atomic weight.	Specific heat.	Product.	
Hydrate of lime, .	4·625	0·3*	1·387	0·375 × 4 = 1·5
Alcohol, ...	2·875	0·62(d)	1·787	0·375 × 5 = 1·875

TABLE VII.—Sextenary Compounds.

	Atomic weight.	Specific heat.	Product.	
Sulphate of barytes,	14·5	0·107‡	1·551	
Sulphate of strontian,	11·5	0·130‡	1·495	
Sulphate of lead, .	19	0·0848‡	1·604	
Anhyd. sulph. of iron,	9·5	0·145*	1·377	
Sulphate of potash,	11	0·169*	1·859	Mean 1·805
Sulphate of zinc, .	10·125	0·213*	2·156	0·375 × 5 =
Sulphate of lime, .	8·5	0·190*	1·815	1·865
Anhyd. sulph. copper,	10	0·18*	1·8	
Anhyd. sulph. of soda,	9	0·263*	2·367	
Hydrate of alumina,	3·375	0·420*	1·424	
Arsenical pyrites, .	20·5	0·1012‡	2·075	
Glance cobalt, . .	20	0·107‡	2·140	
Carbonate of barytes,	15	0·1078‡	1·638	
Carbonate of strontian	12	0·1445‡	1·734	
Carbonate of lead,	19·5	0·0816‡	1·591	Mean 2·077
Carbonate of soda,	9·5	0·306*	2·907	0·375 × 6 =
Carbonate of potash,	11·5	0·237*	2·725	2·25
Carbonate of lime,	9	0·203	1·827	
Carbonate of magnesia	8	0·227‡	1·816	
Magnetic iron ore,	14·5	0·1641‡	2·379	

TABLE VIII.—Octenary Compounds.

	Atomic weight.	Specific heat.	Product.	
Nitrate of potash, .	12·75	0·269*	3·43	
Nitrate of soda, .	10·75	0·24*	2.5	Mean 2·977
Bihydrate of potash,	8·25	0·358*	2·953	0·375 × 8 = 3·
Ether, ...	4·625	0·52(d)	2·405	
Fahlkies, . . .	28	0·1284‡	3·595	

TABLE IX.

	Atomic weight.	Specific heat.	Product.	
10 atoms Zoisite, .	14	0·194‡	2·716	= 0·375 × 7
12 ,, Gypsum,	10·5	0·302‡	3·171	= 0·375 × 8
14 ,, Bitter spar,	11·5	0·2137‡	2·45	= 0·375 × 6
20 ,, Topaz, . .	16·25	0·200‡	3·25	= 0·375 × 9
23 ,, Labradorite,	24·25	0·1926‡	4·67	= 0·375 × 12
23 ,, Hornblende,	29	0·1976‡	5·73	= 0·375 × 15
47 ,, Adularia,	36·75	0·1861‡	6·839	= 0·375 × 18
47 ,, Albite, .	34·75	0·1961‡	6·814	= 0·375 × 18

The specific heats marked by an asterisk (*) were determined by Avogadro.—
See Ann. de Chim. et de Phys. lv. 80 and lvii. 113. and Records i. 108, and ii.
34. Those marked † were determined by Dulong and Petit.—See Annals of
Philosophy, xiii. 164. and xiv. 189. Those marked ‡ were determined by
Neumann.—See Poggendorff's Annalen, xxiii. 1. Those marked § were deter-
mined by Dalton.—See New System of Chemical Philosophy, i. 62. Those
marked ‖ by Lavoisier and Laplace. Those marked (c) by Crawford.—See table
at the end of his Treatise on Animal Heat. Those marked (d) by Dupretz.—
See Ann. de Chim. et de Phys. xxiv. 328.

The first table exhibits the atomic weights and the specific
heats of 26 simple bodies, together with the products ob-
tained by multiplying the atomic weight and specific heat
of each body into each other. This table, by means of
horizontal lines, is divided into 6 compartments. Of these
we shall consider the third group first, which includes 16 dif-
ferent bodies or almost two-thirds of the whole. The product
of the atomic weight of these bodies into their respective
specific heats gives numbers which do not absolutely coincide
with each other; but which, if we leave out the number
for azote, approach each other pretty nearly, and the mean
of the whole gives the number 0·375 as the product of the
atomic weight and specific heats multiplied into each other.
In all of these bodies then we may conclude the *same
quantity of heat exactly is attached to each atom;* and the
reason why their specific heats appear different is that the
atoms of the respective bodies differ from each other. But
the specific heat of each atom of all these bodies is abso-
lutely the same.

Of these 17 simple bodies, there are 8, namely,

Sulphur, . .	0·376
Antimony, . . ·	0·376
Nickel, . . .	0·375
Lead, . . .	0·377
Copper, . .	0·376
Gold, . . .	0·372
Tin, ...	0·370
Platinum, . .	0·372
Mean, . . .	0·3746

that agree so closely in the product of their specific heat multiplied by their atomic weight, that no doubt can be entertained that the atomic weights for these bodies given in the tables are the true ones. The atomic weights of Berzelius for these bodies differ but little from mine ; yet, if we were to adopt his numbers, with the exception of nickel, his number for which I have approximated to in the table, the results would deviate farther than mine from 0·375, as may be seen by the following table in which his numbers are adopted.

	Atoms of Berzelius.	Specific heat.	Product.	
Sulphur, ...	2·01165	0·188	0·378	
Antimony, ...	8·06452	0·047	0·379	
Nickel, ...	3·69675	0·1035	0·383	
Lead, ...	12·94498	0·029	0·375	⎧ Mean
Gold, ...	12·43013	0·0298	0·370	⎨ 0·377.
Copper, ...	3·95695	0·095	0·376	⎩
Tin, ...	7·35294	0·051	0·375	
Platinum, ...	12·33499	0·031	0·382	

The deviation is undoubtedly but trifling, because Berzelius's atomic numbers for these bodies are almost identical with mine. But the reader will perceive, at a glance, how little is gained by the long string of decimals appended to each of his atomic numbers.

There are three of these simple bodies, namely,

Tellurium, . . .	0·364
Bismuth, . . .	0·360
Mercury, . . .	0·3625
Mean, ...	0·3623

which sinks below the mean number 0·375 by 0·012, or about $\frac{1}{31}$. If we take Berzelius's atomic numbers, the result will be seen in the following table.

	Atoms of Berzelius.	Specific heat.	Product.
Tellurium, ...	8·0176	0·0912	0·731
Bismuth, ...	13·30377	0·04	0·532
Mercury, ...	12·65823	0·029	0·367

The reader will perceive, at a glance, that the numbers chosen by Berzelius to represent the atomic weights of tellurium and bismuth cannot be the true numbers. His number for tellurium is almost exactly double mine. Four, there is every reason for believing, is the exact atomic weight of tellurium. Doubtless, there is a small error in the determination of the specific heat. Had the specific heat been 0·0391 instead of 0·0912 the product into the atomic weight would have been 0·375.

It is equally obvious that 13·3 cannot be the true atomic weight of bismuth. The product of this number by the specific heat of bismuth deviates too far from 0·375 to permit us to adopt it. The atomic weight of mercury, as fixed both by Berzelius and myself, differ very little from each other. If the specific heat be accurately determined both are too low; 13 in all probability is the correct number.

Four of these simple bodies, namely,

Arsenic,	. . .	0·385
Iron,	. . . .	0·385
Zinc,	. . . .	0·383
Hydrogen,	. . .	0·412
Mean,	. . . .	0·391

have numbers obtained by multiplying the atomic weights into the specific heats higher than 0·375 by about $\frac{1}{23}$d part. Let us compare them with Berzelius's numbers.

	Atoms of Berzelius.	Specific heat.	Product.
Arsenic, ...	4·70042	0·081	0·381
Iron, ...	3·39205	0·11	0·373
Zinc, ...	4·03226	0·0927	0·374
Hydrogen, ...	0·062398	3·2936	0·205

The first three of these numbers approach nearer the mean quantity 0·375 than mine. But the difference is not sufficient to warrant the conclusion, that Berzelius's numbers are right and mine wrong. Because a very slight change in the number representing the specific heat would bring the products of the atomic weights of arsenic, iron and zinc as I have given them to the mean number 0·375. Were the specific heats as follow :

Arsenic, . . 0·079 instead of 0·081
Iron, . . . 0·107 ,, 0·11
Zinc, . . . 0·0909 ,, 0·0927

the products of these numbers by the atomic weights would be 0·375. Now, these deviations do not much exceed 1 per cent., an error that may be easily committed in determining the specific heat of bodies.

In my original experiments to determine the atom of zinc I obtained 4·2. Now, the number 4·125 which I have adopted here comes almost as near 4·2 as my former number 4·25. And I have preferred it, because, when multiplied by the specific heat of zinc, the product approaches nearer to 0·375 than when 4·25 is taken.

As to hydrogen, the number obtained by multiplying its atomic weight by its specific heat deviates considerably from 0·375. For 0·412 = 0·375 + 37 or $\frac{1}{10}$th of the whole. The reason of this I take to be, that it is much more difficult to determine the specific heat of hydrogen gas with precision than that of any other substance. And there are strong reasons for believing that the number 3·2936 obtained by Delaroche and Berard does not constitute a very near approximation to the truth. Had the specific heat of hydrogen been only 3 instead of 3·2936 its product multiplied by 0·125 would have given the mean number 0·375.

It is obvious, at a glance, that the atomic weight of hydrogen adopted by Berzelius is too small. For, when multiplied by the specific heat, the product does not much exceed one half of 0·375.

When we look at the product of the atom of azote multiplied by its specific heat, it is obvious, that it is about $\frac{1}{4}$th part too high. This I ascribe to the difficulty of determining the specific heat of this gas with accuracy.

Were the specific heat of azotic gas only 0·214 instead of 0·2698, we would obtain the mean number 0·375.

Berzelius's atomic weight of azote is 0·88518. This multiplied by 0·2698, gives 0·2388, which is much farther below 0·375 than my number is above it. It is evident that Berzelius's number is only one-half the true atom of azote. If Mr. Dalton will consider the atom of azote with reference to its specific heat, he will be satisfied that his ingenious arguments in favour of 0·875, for the atom of azote, are not so conclusive as he has hitherto thought them.

Having sufficiently discussed the atomic weights in the third compartment of the table, let us now attend to the three substances in the first compartment, namely, carbon, silicon and aluminum. Of these three, the specific heat of carbon alone has been determined experimentally. I have inferred the specific heats of silicon and aluminum from those of silica and alumina determined by Neumann. They may be seen at the beginning of the second table. And the way in which I have deduced these specific heats will appear when I come to consider the second table.

It will be seen, on inspecting the table, that if we admit 0·75 to be the atomic weight of carbon, the product of that number by its specific heat, instead of being 0·375 is only 0·19275, or about half that number. Shall we conclude from this, that an atom of carbon is united with only half the quantity of heat which is combined with the atoms of the bodies constituting the third compartment of the table? I think it more likely that we have occasioned the anomaly by making the atom of carbon only half its real weight. Let us suppose the atom of carbon to be 1·5 we have 1·5 × 0·257 = 0·385, a quantity which comes very near the mean number 0·375. If the atom of carbon be 1·5, then

		Atoms.		Atoms.	
Carbonic oxide is composed of	1 carbon	+	2 oxygen
Oxalic acid	2	,,	+	3	,,
Carbonic acid	1	,,	+	4	,,
Carburetted hydrogen of .	1	,,	+	4 hydrogen
Olefiant gas	1	,,	+	2	,,
Alcohol of 1 atom oxygen +	1	,,	+	3	,,
Ether of 1 ,, +	2	,,	+	5	,,

And the atomic weights of these bodies would be

Carbonic oxide,	. . . 3·5
Oxalic acid, .	. . . . 4·5
Carbonic acid, .	. . . 5·5
Carburetted hydrogen,	. 2
Olefiant gas,	. . . . 1·75
Alcohol, .	. . . . . 2·875
Ether,	. . . . . . 4·625

These numbers have nothing in them to startle us. The atom of carbonic acid being doubled the salts at present called bi-carbonates would, in fact, be carbonates, and those called carbonates would be di-carbonates. Analogy favours this new notion. The bi-carbonates are much nearer the neutral state than the carbonates; and, therefore, more likely to contain an atom of each constituent. While the carbonates possess all the characters which usually characterize the sub-salts.

Berzelius's atomic number for carbon being almost identical with mine, it is needless to institute a comparison between them.

For the same reason it will be necessary to double my atomic weight for silicon and aluminum; making the former 2 and the latter 2·5. These numbers multiplied into the specific heats in the table would give us for a product the mean number 0·375. But, as the specific heats of these bodies are only hypothetical, it is needless to insist farther on the subject.

The specific heat of oxygen in the second compartment of the table is given according to the experiments of Delaroche and Berard, while those of chlorine and bromine are deduced from the experiments of Messrs. Haycraft, Delarive and Marcet, according to whom, the specific heats of gases and vapours are inversely as the square roots of their specific gravities. It is obvious, that none of these specific heats can be depended on; but, from the numbers in the third column, we see that my atomic numbers must be true ones.

Berzelius has adopted for chlorine and bromine numbers which give the following results:

	Atoms of Berzelius.	Specific heat.	Products.
Chlorine, ...	2·21326	0·0827	0·183
Bromine, . . . ·	4·89153	0·0472	0·231

The first of these products is only half of 0·375. The second is little more than half that number, doubtless, because our estimate of the specific heat is too high. The true atomic weights of these bodies are double the numbers of Berzelius or 4·5 for chlorine and 10 for bromine. Dr. Turner's number for chlorine is double that of Berzelius's. His atom of bromine is 9·8. Both of these are pretty near the truth, but not exactly so.

We have now taken a view of all the substances in the first table except the four last; namely, cobalt, phosphorus, silver and iodine.

The specific heat of cobalt was determined by Dulong and Petit, and they give us no information either respecting the purity of the metal, the mode of operating, or the quantity of metal used in their experiments. The same observations apply to the specific heat of nickel, which was determined by the same gentlemen, and given by them merely in a table without any details whatever. Now, as the atomic weights of nickel and cobalt very nearly agree, while their specific heats, according to Dulong and Petit are as follow,

$$\text{Nickel,} \quad . \quad . \quad 0\cdot1035$$
$$\text{Cobalt,} \quad . \quad . \quad 0\cdot1498$$

there is reason to suspect some inaccuracy in one or other of these determinations, or in both of them. Under these circumstances it is not surprizing that the number resulting from the multiplication of the atomic weight and specific heat of cobalt instead of 0·375, is 0·4875 about ⅓d greater than the truth. Berzelius's atom of cobalt is 3·68991; which multiplied by 0·15 gives 0·553, which deviates still farther from 0·375 than the quantity resulting from my atomic number does.

To clear up the anomaly regarding cobalt, new experiments on its specific heat must be made. Were the specific heat 0·115 instead of 0·1498, cobalt would coincide with the other simple substances.

Phosphorus and silver, when their atoms and specific heats are multiplied together give 0·770 or 0·375 × 2. To bring them to agree with the other simple substances we must reduce the atom of each to one-half; we then have

	Atoms.	Specific heat.	Product.
Phosphorus, . . .	1	0·385	0·385
Silver, ...	6·875	0·056	0·385

Berzelius's atoms for these bodies reduced to one-half, give

	Atoms.	Specific heat.	Product.
Phosphorus, . . .	0·98071	0·385	0·377
Silver, ...	6·758	0·056	0·378

Both of these numbers approach nearer 0·375 than mine. I think it exceedingly probable that the true atomic weight of silver is 6·75. But unity is much more likely to be the atom of phophorus than 0·98071. A very slight alteration in the specific heat of phosphorus as determined by Avogadro would render the product by unity 0·375. We have only to suppose it 0·375.

As for iodine, the number obtained by multiplying its atomic weight by its specific heat is 1·40175, which is four times greater than the mean quantity 0·375. Were we to adopt Berzelius's atom of 7·8975 matters would not be much mended. We would obtain 0·703 which approaches 0·375 × 2. The atom of iodine is obviously heavy. I do not think it advisable to reduce it to one-fourth of its present number or to 3·9375. I think it more probable that iodine will ultimately be found a compound of four distinct atoms, and that its true place is in a subsequent compartment of our table.

Let us now take a view of the second table. It contains 16 substances all of which, in the present state of our knowledge, are considered as binary compounds, or compounds of two simple substances united together. One of these constituents is always either oxygen or sulphur, while the other is a combustible. Besides these 16 there are 5 in a compartment by themselves at the end of the table consisting of water and four different chlorides. Let us examine the 16 binary compounds constituting the upper compartment of the table.

It has been already observed, that to make silicon and aluminum accord with the general law, we must double

their atomic numbers, making that of silicon 2 and that of alumina 2·5. This would raise the atom of silica to 4 and that of alumina to 4·5. Substituing these for the atomic numbers in the table their product by the specific heat gives us 0·716 and 0·874.

The numbers in the third column of the table do not quite agree with each other; but there is such an approximation as to warrant the conclusion, that the deviations are owing to errors committed in determining the specific heats. The mean of the whole 16 gives 0·738 as the product of the atomic weight by the specific heat. Now, 0·375 × 2 = ·750 which approaches pretty near the mean found.

From this it appears that the specific heat of all these binary atoms is the same, and that it amounts to twice as much as the quantity combined with the simple atoms. It would seem from this that *each of the atoms in the binary compound retains all the heat with which it was united previous to its entering into the state of a compound.*

The atomic numbers of Berzelius approach so near my own in all these binary compounds except three, that it is needless to compare them. The three exceptions may be seen in the following little table.

	Atoms of Berzelius.	Specific heat.	Product.
Oxide of chromium, .	10·03631	0·196	1·967
Sulphuret of mercury, .	27·3281	0·052	1·421
Sulphide of arsenic, . .	13·42414	0·1111	1·49

The mean of these products is 1·626, which is rather more than double the average deduced from the 16 binary compounds. But Berzelius considers oxide of chromium as a quarternary compound. Now 0·375 × 4 = 1·5. The other two he makes ternary compounds and 0·375 × 3 = 1·125. Numbers which deviate too far to admit us to adopt Berzelius's atoms as the true ones.

Let us now attend a little to the five binary compounds at the end of table second.

The atomic weight of water multiplied into its specific heat gives 1·125. This number corresponds with that belonging to the ternary compounds. Now, we might consider water as a compound of one atom oxygen and two

atoms hydrogen, which would place it among the ternary compounds. In that case the atom of hydrogen would weigh only 0·9625. This number multiplied into 3·2936, the specific heat, would give us 0·206, which is considerably below the average number 0·375. But this may be owing to an error in the determination of the specific heat of hydrogen. Were we to adopt 4·177 for the specific heat of hydrogen the number calculated from the experiments of Delarive and Marcet, the multiplication of that quantity by 0·0625 would be ·261, which approaches somewhat near the mean 0·375, though still at a considerable distance. In the present state of our knowledge, it would be hazardous to determine whether water be a binary or ternary compound.

Corrosive sublimate is precisely in the same predicament as water. It consists only of two atoms united, and yet the product of its atomic weight by its specific heat is 1·175, which is nearly 0·375 × 3. In the present state of our knowledge I see no other conclusion to which we can come, than that when an atom of mercury and an atom of chlorine unite to constitute corrosive sublimate, the quantity of heat united to each of the constituents, while separate, becomes augmented by one-third when they unite together.

Common salt, chloride of potassium and chloride of calcium are also binary compounds, according to the theory universally admitted by chemists; yet the mean product of their atomic weight by their specific heat is 1·587, which is nearly equal to 0·375 × 4. We must conclude from this, if the presently received chemical theory be true, that when chlorine combines with sodium, potassium or calcium the quantity of heat united to each atom, before combination, is *doubled*.

The third table consists of compounds of one atom of a metal with 1½ atoms of oxygen or sulphur. The mean product of their atomic weight by their specific heat gives 0·847; while 0·375 × 2½ = 0·9375. The difference is, probably, owing to errors in the determination of the specific heat. No light would be thrown on the subject by examining the atomic weights assigned by Berzelius. He doubles his weights in order to get rid of the fraction; and, of course, doubles the number of constituents. In the pre-

sent state of chemistry, I am not aware of any advantage
which attends this doubling.

The fourth table exhibits the atomic weights and specific
heats of 11 ternary compounds. The products differ a good
deal from each other, shewing that the specific heats have
not been determined with accuracy. But, if we leave out
olefiant gas which deviates too far from the rest, the mean
product of all the others gives 1·071. Now, 0·375 × 3 =
1·125 approaching pretty near the mean. Hence, we may con-
clude, that each of these ternary compound atoms contains
*thrice as much heat as any of the atoms entering into its com-
position.* So that in ternary compounds each simple atom
retains all the heat which was united with it when in an in-
sulated or uncombined state.

I am afraid that it would hardly be safe to carry our
computations much farther.

Table fifth contains one substance, chrysolite which is a
compound of four atoms. Now, the product of its atomic
weight by its specific heat is 1·332, which approximates to
1·500 = 0·375 × 4.

Sulphuric acid, the other substance in this table, is a
compound of one particle of real sulphuric acid containing
four atoms, and one particle of water containing two or
three atoms. So that it is a compound of six or seven
atoms according to the view which we take of the constitu-
tion of water. Now, the number which it gives, namely,
2·144, which is not far from 2·250 = 0·375 × 6. So that,
if we reckon water a binary, and sulphuric acid a quarter-
nary compound, all the constituent atoms enter into com-
bination without parting with any of their heat.

Table sixth contains the only two examples of quarter-
nary compounds whose specific heats have been determined.
Now, 0·375 × 5 = 1·878. A number approaching to that
which alcohol yields.

If water be a binary compound, the hydrate of lime is
only a quaternary, and its number 1·387 approaches 1·500
= 0·375 × 4.

The seventh table exhibits the atomic weights and speci-
fic heats of 12 sextenary compounds. Now, the mean pro-
duct furnished by all these bodies is 1·805. But 0·375 ×
5 = 1·865. It would seem from this that the binary atom

constituting the base of these saline bodies loses one half of its heat when it unites with the acid. The quantity of heat instead of being six times as great as in a simple atom is only five times as great. At the same time, it must be acknowledged, that the specific heats of these bodies are so imperfectly determined that it may be too small in them all, except sulphate of soda which seems to contain six atoms each furnished with its normal quantity of heat.

The eight carbonates and magnetic iron ore at the end of table seven are septenary compounds, or contain seven simple atoms united together. Now, the mean number which they give is 2·077. And 0·375 × 6 = 2·250. It would appear from this, that binary bases lose half their heat when they combine with acids; while the acids part with none of their heat.

Table eight exhibits the products of the atomic weights of five octenary compounds multiplied respectively into their specific heats. The mean product is 2·977. Now, 0·375 × 8 = 3·500. Hence, it would appear, that all the atoms entering into these compounds retain the heat with which they were furnished when in an uncombined state.

·Our knowledge of the composition of the eight substances constituting the ninth table is too imperfect to permit us to pursue the investigation. I have added a fourth column, shewing what multiples of 0·375 are obtained when we multiply the atomic weight of each into its specific heat. It might lead to some important conclusions respecting the true constitution of these minerals. But as it would not throw any additional light on the atomic weights of simple bodies, I shall here terminate this paper, which has been drawn out to a much greater length than I contemplated at first.

ARTICLE III.

Notice of the Parr. By Sir WILLIAM JARDINE, Bart.[*]

THE fishes inhabiting the rivers of Berwickshire are comparatively limited in the numbers of their species, but some of them are of so much importance and value, while others, as the little fish we are now about to notice, although

[*] Proceedings of the Berwickshire Naturalists' Club, p. 82.

abundant, and familiarly known as a *parr*, has yet some
unrivalled mysteries in its history, and there are, I believe,
only a few persons at the present time, who are able to
say what it really is, or to point out the distinctions which
separate it from its congeners. Among naturalists gener-
ally, an uncertainty seems to have existed whether this was
the young of some of the migrating salmon; but more
lately, this opinion seems to have resolved itself into this,
whether the *parr* was a species, or only the young or variety
of the common river trout. The following observations are
the result of comparisons made last month between Tweed
specimens of the *parr* and S. Fario. We shall first, how-
ever, shortly notice the habits of the former.

Among the British Salmonidæ, there is no fish where the
habits are so regular, or the colours and markings so con-
stant. It delights in the clearest streams, with rocky or
gravelly bottoms, and seems pretty generally distributed in
Britain in those which have this character; but is not at
all found in the low and flat districts, where the waters are
deep and sluggish. It frequents the shallower fords, or
the heads and lower parts of streams, in shoals, hanging
nearly in one place, and in constant activity from the exer-
tion, apparently day and night. It takes any bait with
the greatest freedom at all times, and when no trout, though
abundant among them, will rise or bite. That part of its
history which is yet unknown is its breeding. Males are
frequently found so far advanced, as to have the milt flow
upon being handled; but at the same period, the females
had the roe in a very backward state. Neither have they
been seen in an advanced state at any other season, or dis-
covered spawning upon the shallower streams, like the
common trout. It is probable that this little fish may also
be found in some of the continental alpine and subalpine
streams, but I cannot say so from observation. In the
north of Europe I suspect it is wanting; and in our late
excursion to Sutherland, a perceptible decrease of its num-
bers was observed towards the north. It should also be
observed, that I have never seen the *parr*, or been able to
find any traces of it, except in rivers which had an uninter-
rupted intercourse with the sea. The size is from *three* to
six inches in length; very rarely specimens reach eight

and nearly nine inches. It abounds in nearly all the Berwickshire rivers.

The general colour on the upper parts is a greenish-grey, changing to pure silvery white on the lower parts, which, however, are sometimes tinted with yellow. When the streams which they frequent are impregnated with moss from some of the small alpine sources, upon each side is a row of oval-shaped marks of a deeper tint, and more inclining to bluish-grey than that of the upper parts; and it is probable, that from a somewhat similar marking being seen in the young of the common trout, and the young of several other Salmonidæ, the supposition of this being identical with some of them was first surmised. In the *parr* these markings are narrower and more lengthened in their form. The general smaller spotting of the sides seldom extends below the lateral line, and upon the gill-cover there are almost always two black spots; sometimes one is only distinctly marked, but a trace of the other is perceptible, and the relative position of them is almost always alike. In comparison with a trout of similar size, the *parr* is altogether more delicately formed; the nose is blunter; the tail more forked; but the chief external distinction is the great comparative power of the pectoral fins, which are longer, much more mascular, and nearly one-third broader; and we shall at once see the necessity of this greater power, when we consider that they serve to assist in almost *constantly suspending* this little fish in the most rapid streams. Scales of the *parr*, taken from the lateral line, were altogether larger, the length greater by one-third; the furrowing more delicate, and the form of the canal not so apparent or so strongly marked towards the basal end. In the osteology of the head, which appears to offer the most constant and well-marked distinctions in this tribe, the general delicacy (still continuing the comparison with a trout of same size) of the bones is in all parts kept up. The *opercle*, forming the posterior edge of the gill-covers, is much more rounded, approaching, in this respect, to the form of it in the salmon; in the trout, the lower corner is decidedly angular. The *inter opercle* is longer and narrower. The *maxillary bone* is much shorter, but broader at the posterior end, whence the much shorter or less gape in the *parr*. The *vomer* is much

weaker. The bones of the rays of the gill-covers are longer
and much narrower. The tongue is longer, weaker, and
not so broad. The under jaw much weaker, and the dis-
tance between its rami one-third less. The *teething* much
more delicate.

These are the principal distinctions which appear in the
external form and osteology of the head. In the latter
every bone differs, and the differences appear constant in
all that I have taken to pieces; and in this state, therefore,
I have no hesitation in considering the *parr* perfectly dis-
tinct from any species or variety of trout we are acquainted
with, and entitled to hold a separate rank in our Fauna.
From the *Migratory Salmon* it is separated, entirely by its
habits; and, I consider it should therefore stand in our
systems as the *Salmo Salmulus* of Willughby and Ray. The
correct distinguishing marks to be seen by a person who
has not leizure to make a minute examination, are the
great size of the pectoral fins, the shortness of the maxillary
bones, and consequent small gape, and the narrow breadth
between the rami of the lower jaw.

ARTICLE IV.

On a very powerful natural Magnet. By Mr. J. CRICHTON,
Glasgow.

To the Editor of the Records of General Science.

SIR,—The extremely small loadstone which belonged to
my deceased father, and of which you desire to receive
some details, is perhaps the most powerful of its kind ever
known. By the scientific reader it will probably be regarded
as a very curious and rare production of nature, while by
some others it may be thought to possess interest of a dif-
ferent description. Since no other person, now alive, knows
any thing of its history, I shall, from my own immediate
knowledge and circumstantial recollection, give the best
account of its origin, which at this time, it is possible to
put on record.

In 1772, or the succeeding year, when Benjamin Franklin
was in Glasgow, he called on the late Professor Anderson.
Much of the conversation which took place between them

was on electricity; both were enthusiasts in this branch of science. And, at their joint representation, the thunder-rod, still on the College steeple here, was erected with a view to its protection from the effects of lightning.

Magnetism also became the subject of discussion, in the course of which, the Professor desired my father, who at that time lived in the Professor's house as his mechanist, to exhibit some artificial magnets he had just finished. On this occasion Franklin mentioned, on the authority of his friend Washington, that some place in Virginia afforded very fine loadstones, and added, that on his return to America, he would endeavour to procure a specimen and send it to the Professor.

This was not neglected, for in 1776, the Professor received the promised mineral, which was probably brought to France by Dr. Franklin, whence he transmitted it to Glasgow as a present from Washington himself. The most promising portion of the mass was selected, and my father then working on his own account, was employed to arm it in the most approved manner; but, though this was carefully performed, its power was in no way remarkable. Several smaller portions of the mass were similarly fitted up; these, however, like the principal one, proving almost value-less, the Professor declined making further trials, and finally laid aside all thoughts of the matter.

Some years afterwards, I think in 1781, my father, casually rummaging a lumber-box which stood under his work-bench, perceived some small fragments of the almost forgotten loadstone surrounded by iron filings and other ferruginous dust, and remarking that one of these frag-ments carried a larger *beard* of filings than the others, he was thereby induced, at his first leisure, to bestow, what he then thought, a little hopeless labour in grinding it to a proper shape with due regard to its poles. The diminutive iron arms were attached in a temporary manner by means of a thread, when, to his great surprise, its first load, though hastily applied and supposed to be in excess, required sensible force for its removal.

It now seemed worthy of some additional labour; the form in regard to polarity was re-examined, and when finished in this respect, the stone, after being weighed, was

with its arming enclosed in a thin case of gold, having a ring at top for suspending it. Its load, a pyramidal shaped piece of soft iron, was now made of what was judged a weight rather under its maximum power, that is, 783 grains; the stone itself weighs precisely two and a-half grains; it carries, therefore, 313 times its own weight.

It is now about 55 years since this little spark of the mine was first enclosed; upwards of 30 years ago the case was opened, in order to apply arms of perhaps a better shape; the old ones, however, appearing in all respects faultless, the whole was immediately put together in its original state.

Scientific indviduals have frequently suggested the propriety of keeping it with the load constantly attached, as a mean, they allege, of increasing its strength. This, I apprehend, is rather a gratuitous assumption; besides, constant adhesion could not be maintained owing to the tremors incessantly taking place in every dwelling house. Though it is not doubted, that, by careful application, the load could be increased to considerably more than 800 grains; still, as there is reason for thinking that violent separation of the load, under such circumstances, might prove injurious, the trial has never been made.

The same mass of iron has been used as its load from the beginning, and is placed merely in contact with the arms. The power of adhesion seems to be the same as it has ever been.   JAMES CRICHTON.

Glasgow, 1st March, 1836.

ARTICLE V.

The Art of Dyeing.

(*Continued from* page 218.)

Test of the Potash.—We cannot perceive whether the potash is good or bad. This is determined by means of sulphuric acid. The more sulphuric acid potash requires to saturate it, so much the better is it. The following process should be adopted: Take any measure, one for ex-

ample, which may contain ½ loth (·234 Troy ounce) water, and measure with it any flask, in order to ascertain how many of such measures it can hold; 90 such measures are now to be added, and 90 grans (86·3 grs.) of sulphuric acid of the sp. gr. 1·850 accurately weighed out. Pour some water out of the flask and agitate the sulphuric acid in it. Wash the vessel well out with water so as to lose no sulphuric acid, and then add this with as much water as will again fill up the flask.

Several loths (half-ounces) of the potash to be tested are now to be well rubbed in a mortar, 10 grans (9·59 grs.) weighed out, dissolved in a little water and mixed with some tincture of litmus. When this is done, measures of the above mentioned sulphuric acid test solution are to be added gradually, until the blue solution of potash begins to become red. If the red colour remains after the solution is heated, it is a proof that all the alkali in the potash is saturated with sulphuric acid, and the quantity of sulphuric acid solution is now to be estimated. Since now each measure indicates a grain of sulphuric acid, so we calculate according to the quantity of sulphuric acid employed, the quantity of alkali contained in the potash, or what is the same thing, the strength of the potash. Thus, 49 grans (47· grs.) of sulphuric acid indicate 47½ (45·5 grs.) of pure potash.*

A more convenient test of the potash for the dyer is this. Several ells† of cotton dyed chemical blue,‡ the process for which will be subsequently described, are to be rolled up, and laid uniformly together; equally large pieces are then to be cut out from it with a hollow chisel. A good specimen of potash is then to be taken, and 1 gran (·959 gr.) dissolved in 300 (287·7 grs.) pure water; heat the solution in a porcelain basin over a spirit lamp, and put into it one portion after another of the chemical blue cotton, in such a manner that the first shall have lost its colour before the succeeding one is introduced. At last a point will be attained when the cotton will no longer be deprived of its colour, but will retain its blue colour.

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* Or more correctly, 5 grs. sulphuric acid indicate 6 of caustic potash.—EDIT.
† The Berlin *elle* is equal to 666·9 millimetres or 2·188 English feet.—EDIT.
‡ Records, vol. i. p. 323.

The testing is now completed. The portions of cotton which have been decolourized are to be counted. Should they amount to 6, it follows that 6 chemical blue pieces of cotton to be deprived of colour require 1 gran of good potash. If another specimen of potash is now examined, and it is found that 1 gran of this can decolourize only 3 pieces of the same cotton, it follows that the last is only half as good as the first specimen.

It is obvious, that in this method of testing, the same cotton must always be employed which served at first for the examination of the good potash.

If it is wished to determine in this way the actual quantity of potash in a ley, the first trial, upon which the estimate in future trials depends, should always be made with 1 gran (·959 gr.) of pure potash instead of the ley.

For a correct estimate, let ½ loth or 120 grans (115·08 grs.) of potash be weighed out, and dissolved in 59½ loth (27·9 oz.) of water; ½ a loth (·234 oz.) of this solution which may be employed for examination contains 1 gran (·959 gr.)

Carbonate of Soda.—Crystallized carbonate of soda possesses a similar action to potash. It is only used, however, when the potash cannot be employed in consequence of its containing foreign salts; for example, as an addition to saturate the acid of the aluminous mordant, and to clear madder-red by depriving it of the trace of yellow.

In the exhibition of the oil mordant the potash is used. Carbonate of soda does not answer.

Carbonate of soda can be rendered caustic like potash, or can be deprived of its carbonic acid by means of lime. It should be observed, that, in general, 143 lbs. crystallized carbonate of soda and 120 lbs. of good potash bear the same proportion to the lime employed.

Carbonate of soda does not become moist in the air, but effloresces into a white powder. This renders it fit for printing. A case seldom occurs, however, where it is proper except in a colourless ground of chemical blue, to discharge the yellow pattern. The thickening is produced by starch gum.

As carbonate of soda is often mixed with glauber salt which deteriorates it, it is necessary to submit it to chemical examination. The soda should be tested in the same

way as potash. It is to be remarked, that 49 gran (46·9 grs.) of sulphuric acid require 143 gran (137·1 grs.)*

The chemical blue calico may be used for the same object; hence, a known quantity of pure carbonate of soda gives a standard to judge of the other kinds. For this purpose a solution of 2 grans (1·918 grs.) of pure crystallized carbonate of soda are heated with 300 grans (287·7 grs.) of water and as many pieces of cotton are to be added as shall be decolourized. In other respects the proceeding may be conducted as with potash.

Ammonia.—Occurs in commerce under the name of spirit of salmiac, forming a clear solution with a pungent odour.

When solution of ammonia is placed in an open vessel in the air or heated, the ammonia is dissipated and pure water remains. Ammonia, therefore, although it has almost all the properties of potash and soda, should not be employed when it can be volatilized.

Hence, ammonia does not answer for printing alone. As it dissolves many substances, for example, oxide of copper; it is employed to bring it upon calico, in which case the ammonia is volatilized and the copper colour remains upon the calico.

If we blow tobacco smoke upon paper coloured with Berlin blue it acquires a reddish colour. This proceeds from the ammonia which is contained in tobacco smoke. Here then ammonia itself produces the change of colour. Chemical blue calico also acquires a violet colour from ammonia. In stables where much ammonia is disengaged the same action is always produced.

A calico manufacturer once sent some chemical blue goods packed up in a box, the empty portion of which was filled up with straw. The whole of the goods in the vicinity of the straw were found when unpacked to have become as blue as a violet. It turned out that the straw had been taken from a stable where it had been in contact with urine and other substances containing ammonia.

In printing rooms where mordants containing ammonia, for example, ammoniuret of copper, are printed, no chemical blue goods should be allowed to hang.

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* The atomic numbers are 50 parts sulphuric acid of sp. gr. 1·847 to 146·9 carbonate of soda.—EDIT.

Since the violet blue is a very agreeable colour, so the above observations suggest a method of imparting it to chemical blue. Mix, therefore 1 lb. of solution of ammonia with 300 lbs. water, pass the goods through it, and allow them to dry without washing them.

Much ammonia destroys chemical blue like potash and soda.

As ammonia is an alkali, it prevents the action of acids. It removes, therefore, spots which have been produced upon coloured cotton by vinegar, lime juice, sulphuric acid, &c., and is, therefore, useful in practice as it produces no spot itself; but, in consequence of its volatility, soon disappears. It is to be observed, therefore, as was noticed with vinegar, that we can only expect a result from the use of ammonia when the spot is not too old and cannot be washed out.

Lime.—Quicklime evolves heat in contact with water, and becomes slaked lime. In this state it dissolves in water. Lime-water contains but very little lime. The quantity dissolved diminishes with the heat of the water. Thus, 656 lbs. of water at 32° dissolve 1 lb. lime, and 753 lbs. water at 59° dissolve only a lb. of lime, while of boiling water 1280 lbs. are required for the same quantity of lime.

As heat is disengaged in slaking lime, we must, in order to obtain strong lime-water, add the remaining water after cooling, or place the whole mixture in a cool place and stir it frequently.

Calico can without great risk be boiled with lime-water or milk of lime, when it is carefully washed, and then passed through an acid to remove any lime still adhering to the fibres. Otherwise the lime makes the fibre rough, brittle and tender.

Lime especially injures calico under the influence of light. If a portion of calico is saturated with lime-water, laid for some days in the sun, and again saturated and laid out three or four times, the stuff becomes remarkably white, but so tender that it can be pulled to pieces with the fingers. Lime acts upon most colouring matters like mordants.

Calico which contains lime is therefore dyed as if it were mordanted. Such calico cannot be employed to form a white base out of the same ground, unless the lime is

removed. This is done by digestion in dilute sulphuric acid. Lime decomposes sulphate of iron, and dissolves indigo with its assistance. Upon this depends the exhibition of the cold vat.

If we dissolve yellow chromate of potash in water, boil it and introduce into it a portion of cotton impregnated with the sugar of lead mordant, the cotton immediately assumes a clear yellow colour. If the same experiment is repeated, only using lime-water instead of common water, the colour produced is not yellow but orange. Upon this depends the production of chrome-orange.*

Also other colours, viz., wood colours (Fernambuc, logwood, yellow wood) are made darker by the lime-water which makes the red blueish. It is not, however, to be recommended as it makes the colours dull and coarse.

Lime-water is also used to moisten the cotton which is to be dyed blue in the cold vat.

Chalk is a combination of lime with carbonic acid. It is quite insoluble in water, and, is therefore, without action upon cotton.

When chalk comes in contact with acids it combines with them and destroys their action. This property renders the chalk a useful assistant in washing some calicoes which are printed with acid dischargers. An excess of lime is, therefore, mixed with water, and the cotton passed through this chalk-water before it is rinsed. As the chalk here immediately combines with the acid and separates it from the cotton, no injury can follow. Hence, these patterns preserve their regular edge. It is well in most cases to have the chalk-water, luke warm.

Alum is employed for the exhibition of the aluminous mordant. To obtain by means of it a pure yellow or red colour, it is absolutely necessary that it should be free from iron. As all the alum which occurs in commerce is not free from iron, and, as the iron contained in alum cannot be seen, it is necessary to have recourse to a test for it.

This consists in placing a piece of the alum to be tested in a solution of 1 loth (·469 oz.) Prussiate of potash in 200 loths (93·8 oz.) of water. If the colour of the surface of the alum remains unchanged, the alum is free from iron.

* See a Specimen of this colour.—Records, vol. i. p. 18.—Eᴅɪᴛ.

If, on the other hand, blue spots appear, the alum contains iron. This test is quite certain when the alum is introduced in pieces into the solution. Powdered alum and alum solution cannot be tested in this way with certainty.

It is often difficult to obtain alum free from iron. The dyer may employ the following method advantageously. Let 100 lbs. of alum and 150 lbs. water be boiled in a tinned vessel, and be poured off into a wooden vessel which contains 150 lbs. of cold water, and be agitated until the whole is cooled. Alum free from iron precipitates like flour, while a portion remains in solution with the iron. If the alum-flour is now dissolved in water and the solution allowed to cool, pure alum is obtained in crystals. This will not be rendered blue by the addition of Prussiate of potash.

The mother liquors which remain in both cases are employed for the exhibition of mixed mordants to dye brown, olive or gray.

The alum dissolves copper, and gives dull colours. Copper vessels must not, therefore, be employed to dissolve it. Alum also takes up tin. This is, however, rather useful than hurtful to the dye.

If we boil copper in a solution of alum, wherein tin is soldered, no copper is dissolved; but tin is taken up. We can therefore protect colours containing alum when boiled in copper, thoroughly, from the injurious action of the copper, by soldering a piece of tin in the inside of the vessel.

The solubility of alum in water is not great. It requires 13 lbs. of cold water, at 54°$\frac{1}{2}$, to dissolve 1 lb. of alum. This is the case especially in winter when the water is colder, and dissolves still less. There is, however, a method of remedying this disadvantage to the dyer. This is done by taking away from the alum a portion of its acid. Dissolve 32 lbs. alum in 80 lbs. boiling water, and add gradually 4 lbs. chalk-powder suspended in 80 lbs. of hot water, and heat them until effervescence ceases. A small quantity of alum only separates on cooling, while, without the addition of chalk, 16 or 20 lbs. of alum are separated on cooling from the hot solution. If 11 lbs. of crystallized carbonate of soda are employed instead of the chalk, scarcely any alum separates during the first 24 hours. The acid which the alum contains is the sulphuric. Hence, the alum may

˙prove injurious to cotton fibre. If cotton impregnated with
a solution of alum is allowed to hang up for some time, the
cotton is brittle. Alum saturated with chalk or soda acts
less injuriously.

Sulphate of copper or blue vitriol is of little use to the
cotton dyer. As a mordant it cannot be employed by itself,
as it does not combine with the cotton fibre. If cotton is
saturated with a solution of sulphate of copper and dried,
the whole of the sulphate of copper is removed by washing.

Liquid ammonia dissolves sulphate of copper, forming
an azure blue colour, and affording a useful copper mordant
for many purposes.

With decoction of logwood, sulphate of copper affords a
very remarkable combination which can be employed in
block-printing.

Sulphate of copper almost always contains iron. This is
determined by liquid ammonia into which a solution of
sulphate of copper is poured, so that after being stirred, a
smell of ammonia is given off. If yellow flocks remain un-
dissolved, they are peroxide of iron. Very small quantities
of iron separate in the course of 24 hours.

Sulphate of iron or green vitriol is an essential ingre-
dient in the cold vat. But every kind of sulphate of iron
is not equally good. Such must be chosen as appears yel-
lowish green, but not yellow. It must also contain no
copper. This is ascertained by means of a piece of polished
iron. If the latter does not change its colour after being
immersed for some hours in a solution of sulphate of iron,
then the vitriol contains no copper. If it acquires a red
coating the presence of copper is indicated. Such vitriol
may be purified from copper by boiling it with iron nails.

In contact with air and cotton the sulphate of iron under-
goes a change which the manufacturer should be acquainted
with, as it explains the numerous hybrid colours obtained
with the iron mordant.

If we place ½ an ell (1·09 feet), of calico in a flask, and
moisten it with a solution of 4 loths (1·876 oz.) sulphate
of iron in 16 loths (7½ oz.) water, pour off all the pure
solution, and place the flask inverted in water, so that its
mouth shall penetrate about an inch below the surface, it
will be found that the water ascends. Hence, it is obvious,

that the calico saturated with the solution of sulphate of iron absorbs a portion of the air contained in the flask, and water takes its place. The cotton which was previously colourless now assumes a yellowish colour. As a piece of calico saturated with a solution of sulphate of iron undergoes the same change in the open air, when it is yet cold and slightly moist, if the cotton has not been too rapidly dried, it is evident that it is the air, which, in both cases, produces the change upon the solution of sulphate of iron.

These properties render it impossible to employ sulphate of iron itself as a mordant with good effect; as all those places which remain in contact with it longer than others, or are more exposed to the air, exhibit from the assistance of the air more iron, and acquire a darker colour.

The acetate of iron, prepared from iron and vinegar, or, from sulphate of iron and sugar of lead, undergoes the same change. Pyrolignate of iron is, however, less changeable.

The sulphate of iron, as well as the acetate of iron mordants which were prepared by means of sulphate of iron, are not fit for mordanting calico, in order to produce a colourless ground.

On the other hand, they are very useful for first printing (vordruck.)

In order to prevent the inconveniences now mentioned, the solution of iron vitriol should be saturated with air before being employed as a mordant, and for this purpose should be placed in a flat vessel exposed to the air, or it may be poured from one vessel into another in a small stream. Both methods are, however, tedious. The object in view is sooner attained by roasting the sulphate of iron till it becomes yellow.

Sulphate of iron thus roasted leaves behind, when dissolved in water, a quantity of oxide of iron, and contains much free sulphuric acid. It will, therefore, prove injurious to the calico unless it is converted into an acetic acid salt by means of sugar of lead.

If iron alum, however, can be procured, it is best to employ it for preparing the iron mordant. The object is thus obtained without difficulty, and with precision. This will be farther treated of afterwards.[*]

[*] See a specimen of Iron Buff, Records, i. 17.—Edit.

Sulphate of manganese has only lately been employed by manufacturers for obtaining bistre. If to a solution of sulphate of manganese we add caustic ley, a brownish white precipitate falls, which, by exposure to the air, becomes dark brown. This precipitate constitutes bistre. To fix it on the cotton, the latter is dipped in a solution of sulphate of manganese, dried and passed through strong caustic ley. At first the cotton acquires but little colour. After some time it becomes dark brown.*

Acetate of lead.—Good sugar of lead should be completely white, afford an acid re-action and give a red colour to blue litmus paper.

Sugar of lead consists of acetic acid and oxide of lead. It decomposes all sulphuric acid salts, and changes them into acetates. If solutions of sugar of lead and alum be mixed together, a white preciptitate falls. This is sulphate of lead produced from the sulphuric acid of the alum and the lead of the sugar of lead. The acetic acid of the sugar of lead remains in the supernatant liquor in combination with the alumina of the alum, and forms a solution of acetate of alumina.

Solutions of iron and copper vitriol, when mixed with a solution of sugar of lead, give the same precipitate of sulphate of lead, as both are sulphuric acid salts.

In the former case, the supernatant liquor contains acetate of iron, in the latter, acetate of copper in solution.

In this way, by means of sugar of lead, all soluble sulphuric acid salts may be converted into acetates.

As sugar of lead is easily soluble, it may be agitated with the solutions of the sulphates, so as to decompose them by stirring them sufficiently long.

Acetate of lead produces no injurious effect upon calico.

In situations where animal substances putrify, for example, near emunctories, stables and dunghills, this salt is covered with a blackish gray coating. This proceeds from sulphur contained in the putrifying bodies which forms sulphuret of lead. Calico impregnated with sugar of lead mordant must be kept away from such situations.

Acetate of soda decomposes sulphuric acid salts like sugar of lead, but with a less favourable result, for the soda

* For specimen of Manganese Bronze, see Records, i. 17.—EDIT.

forms with the sulphuric acid glauber salt or sulphate of soda, which does not separate, but remains in solution.

This circumstance often impedes the manufacturer, and renders it impossible to employ a strong acetate of alumina mordant of alum and acetate of soda, as such a mordant does not thicken well with starch. The glauber salt contained in it destroys the pasty nature of the mordant and makes it watery. On this account a pattern printed with it will not preserve a regular edge.

Acetate of soda is, however, very useful for the preparation of mordants which are not thickened with starch, but with gum or tragacanth, as for example, with acetate of tin formed of tin salt and acetate of soda.

As the salt of tin is a muriatic acid salt, it is improper to employ sugar of lead for the purpose of converting it into an acetate, as the tin mordant will by that means be rendered impure by the presence of muriate of lead which affects the purity of the colour.

By the use of acetate of soda, on the other hand, common salt is produced, which has no injurious action on the dyes.

Acetate of lime or pyrolignate of lime now occurs in commerce for the purpose of being employed instead of sugar of lead. When a solution of it is mixed with one of alum, gypsum precipitates, and the supernatant liquor is acetate of alumina. This contains still much gypsum dissolved in it, and it cannot, therefore, be employed for many colours, as for example, madder-red.

Salt of tin as employed in chemical manufactures occurs in yellowish white crystals, which form a clear solution with a small quantity of water, but with much water give a milkiness which settles into a white precipitate.

Salt of tin consists of muriatic acid and tin, and produces, when printed upon cotton in a strong solution, injurious effects if allowed to remain in contact with it for any length of time. As, however, salt of tin, employed as a discharger, quickly produces its action, the cotton may be washed after the printing, and the injurious effects prevented.

Salt of tin is an important ingredient for yellow and red block-colours. It is better to employ, in this case, acetate of tin, which may be formed by double decomposition with acetate of soda and muriate of tin. For this purpose, we

mix 104 loths (46·9 ozs.) salt of tin, with 136 loths (63·78 ozs.) of crystallized acetate of soda.

It is improper to keep any stock of acetate of tin, as it is very readily decomposed, but it should be first formed in the dye. The salt of tin, should, therefore, be mixed first, and immediately before printing, the necessary quantity of acetate of soda should be added, and both stirred well together.

If a solution of tin be mixed with alum, decomposition ensues, and sulphate of tin precipitates in the form of a white powder. Both should not, therefore, be employed together. Should this, however, happen, the solution of the salt of tin may be added before the sulphuric acid, in the proportion of 104 loths (48·77 ozs.) salt of tin, and 25 loths (11·7 ozs.) sulphuric acid. Sulphate of tin is formed, which is miscible with the solution of alum. When salt of tin is mixed with a solution of soap, the tin separates in combination with the oil of the soap. Notwithstanding this decomposition, this mixture affords a very useful solution for clearing madder-purple and Turkey-red. The proportions are 4 loths (1·87 oz.) to 3 or 400 loths (187· ozs.) oil soap, which should be dissolved in a great quantity of boiling water before adding the salt of tin.

The mordant which occurs in this treatise, under the name of tin mordant, No. 1, consists of 10 lbs. salt of tin, 10 lbs. water, and 10 lbs. muriatic acid. It is an excellent discharger of colourless grounds, especially, of iron mordants.

If we dissolve 104 loths (48·77 ozs.) of salt of tin, in 200 loths (93·8 oz.) water, and saturate the solution by a stream of chlorine gas, a salt of tin is formed, called per-chloride of tin. It answers equally well as a discharger, (Tin mordant, No. 2), but, especially, as an addition to colours printed by the block. This per-chloride of tin, may be converted by means of acetate of soda, like salt of tin, into a less corrosive acetate of tin.

As the per-chloride of tin contains twice the quantity of chlorine contained in the salt of tin, it is necessary, in order to convert it into an acetic acid salt to employ twice as much acetate of soda. We must, therefore, add to the above quantity 272 loths (127½ ozs.) In this case also, the necessary quantity of acetate of soda, should be added, immediately before applying it to the print.

Prussiate of potash.—This salt, constituting a well-known

article of trade, is termed also ferro-prussiate of potash, and is' employed for producing light blue colours upon calico, usually termed chemical blue.*

This blue is nothing else than a Berlin or Paris blue, intimately united with the calico, and is formed when the calico impregnated with the iron mordant is dipped in a solution of prussiate of potash.

In order that this salt may exhibit a sure and rapid action, sulphuric acid must be added. When it does not dye without this addition, it proceeds from the mordanted cotton having been previously washed in hot water.

The proportion of sulphuric acid is important. If a small quantity only is used, the blue does not adhere to the cotton, but is soluble in water, and is washed off by the rinsing. On a white ground it spreads and gives it a light blue colour. The best proportion is, 2 lbs. prussiate of potash, and 1 lb. sulphuric acid.

The solutions of both in water must be mixed while cold. If this mixture is allowed to stand long it is destroyed; the prussic acid is disengaged, and a blue precipitate of Berlin blue falls to the bottom. No more should be prepared, therefore, than is required.

If a piece of cotton dyed with chemical blue be dipped in chlorine water or chloride of lime, it not only remains unchanged, but rather gains lustre. The same happens in the light of the sun. This property makes this colour of great importance in the manufacture of madder-purple, where only such dyes can be used as withstand the action of chloride of lime, and the light of the sun. Indigo and logwood colours want this power. Ferro-prussiate of potash is not poisonous. When it is heated with sulphuric acid solution, the vapour disengaged is, however, deleterious, as it contains prussic acid.

When large quantities of prussiate of potash are poured from one vessel into another, a powder is formed, which smells of prussic acid, and is very troublesome to the nose and throat.

If a solution of prussiate of potash is mixed with a solution of green vitriol, a white precipitate is produced which becomes light blue in the air. A very strong solution of chloride of lime heightens its lustre and makes it finer.

* See a specimen, Records i. 324.—Edit.

With iron alum, or with a solution of nitrate of iron, prussiate of potash gives a dark blue precipitate, termed Diesbach, or Paris blue. It is equally useful with the preceding in block-printing, and when mixed with tartaric acid and printed, it acts as a blue discharge for madder-purple. With copper salts this salt gives a brownish-red colour which does not withstand soap. It is equally good, and more valuable to prepare it with catechu and chips of mahogany.

Chromate of potash.—There are a yellow and a red chromate of potash. The yellow is employed to produce chrome-orange, and the red to form chrome-yellow.

When an acid is added to the yellow-chromate of potash it is converted into the red. If one is only in possession of the yellow-chromate of potash, it may readily be converted into the red and into yellow colours by the addition of an acid. For this purpose, nitric acid is employed. Upon 99 lbs. yellow-chromate of potash, pour 60 lbs. nitric acid of the specific gravity 1·288.

As an acid converts the yellow chromate of potash into the red, so on the other hand, an alkali changes the red into the yellow. A clear solution of potash is here very proper. Crystallized carbonate of soda may be employed for the same purpose. To 151 lbs. of red-chromate of potash add 143 lbs. crystallized carbonate of soda.

The red-chromate of potash is but little liable to adulteration, the yellow is more so. The most correct test for the dyer, is the quantity and quality of its dyeing power. The proportion given under chrome colours, affords a standard for judging of the goodness of a chrome salt.

Chloride of lime as it occurs in commerce is of very different strengths. As it consists of chlorine and lime, and is in the form of a dry powder, it must necessarily contain an excess of lime, and is easily adulterated by means of lime. As this excess of lime separates by solution in water, the value of the chloride of lime may be estimated according to the quantity which remains undissolved, when the dissolved part is chloride of lime. This may also be muriate of lime and the like.

Good chloride of lime must be dry and a little translucent, must bake together and produce no dust on being

stirred. Although chloride of lime may possess all these properties it is not sufficient for the manufacturer. We must test its bleaching power, for this is the property which gives it its value.

There are many chemical tests for this purpose, but they are too troublesome to be employed with advantage by the printer and bleacher. The following test effects the object more quickly : a mordant is prepared of 4 loths (1·87 oz.) alum, 2 loths (·938 oz.) sulphate of iron, 6 loths (2·81 ozs.) sugar of lead, and 32 loths (15 ozs.) water. Some calico is now moistened with the clear solution, washed in water after hanging 48 hours in the air, and dyed with cochineal. The calico acquires a reddish-brown colour, and affords an excellent means of testing the bleaching power of the chloride of lime.

This reddish-brown colour from cochineal will be destroyed by the chloride of lime, and so much the more rapidly and completely in proportion to its strength. If we bring, for example, a drop of the solution of good chloride of lime, consisting of 1 part chloride of lime in 100 water, in contact with the calico, a whitish-yellow spot is immediately produced and the colour is dissipated. If the solution of chloride of lime is still more diluted, as for example, 1 chloride of lime with 200 water, a yellow spot is produced upon the cotton by the contact of a drop, but is less pure and regular. By a proportion of 1 chloride of lime and 300 water less effect is produced, and by greater dilution a point is attained where no remarkable action ensues. This lies in good chloride of lime with a proportion of 1 chloride of lime to 1000 water, while, from this a drop, when placed on cotton, produces a sensible bleaching effect.

It will be readily observed, that by some practice, this procedure affords a sure and easy method of estimating the strength of chloride of lime. But it is desirable, in order to go properly to work, to make a scale of the cotton by means of chloride of lime, while solutions of the chloride are made in 100, 200 and 300 parts of water, a drop of each solution placed upon the cotton, and the latter reserved for future trials.

(To be continued.)

Notice of some Recent Improvements in Science.

HEAT AND LIGHT.

Extrication of light in the formation of crystals.—Rose took from two to three drachms of glassy arsenious acid and digested it in a colourless glass vessel, with 1½ ounces of not fuming muriatic acid of |the usual strength, and half an ounce of water; he brought the mixture to the point of ebullition, and allowed it to remain at this temperature for 10 or 15 minutes, and then cooled it as slowly as possible, by gradually withdrawing the spirit lamp which had been employed in the boiling. When the crystals began to form in a dark place, it was attended with a strong light; the production of every small crystal being accompanied with a spark. If the vessel be shaken, numerous crystals suddenly shoot out, and a corresponding number of sparks is observed. When the quantity of arsenious acid amounts to an ounce or an ounce and a half, with the quantity of muriatic acid already mentioned, on being shaken, the evolution of light from the crystals formed is so powerful, that a dark room is thoroughly lighted.

. When the hot solution of arsenious acid is allowed to cool rapidly, so as to deposit a pulverulent mass of arsenious acid, little or no light is developed. The same happens when the glassy acid is treated with nitric or acetic acid, because these acids dissolve very little arsenious acid. A faint light is observed with sulphuric acid. When a great quantity of arsenious acid is treated with aqua regia, a strong light appears during cooling.

Rose explains this phenomenon by stating, that there are two *isomeric* forms of arsenious acid, one translucent and glassy, the other opaque and like porcelain. In each of these states the acid has a different specific gravity, and a different solubility in water. He conceives that by the action of the acid in the case detailed, the glassy acid is changed into the opaque state; and that by the formation of the new substance in a crystalline form the appearance of light is produced.

This would require further investigation, because the

researches of Mr. Graham have clearly proved that there is no such law in nature as *isomerism*, and that the term should be dropped, unless we choose to designate every new fact by a Greek compound. There is an amusing discussion in the January number of the *Journal de Pharmacie*, between Couerbe and Pelletier, in which the absurdity of the doctrine is exposed. The former asserts, that C H is not isomeric, with $C^2 H^2$, while the latter affirms, that it is so, and laughs at the contrary idea. The truth is, they may both be merry. There seems little doubt, that the different forms of the arsenious acid depend upon the relations of that substance to water.

The same phenomenon of the disengagement of light had been observed before, during the crystallization of sulphate of potash. Rose supposes that the salt was obtained from the residue after the distillation of nitric acid, and was, therefore, sesqui-sulphate of potash, which dissolves in that state in water; but, when it separates from its solution, is converted according to Phillips into bi-sulphate and sulphate of potash.*

Specific heat of salts soluble in water.—Rudberg has furnished a formula for determining this important question.

Let M represent the water in which a salt is dissolved, T its temperature, m, t, c respectively the mass, temperature and specific heat of the salt, that of the water taken as unity, r the temperature of the liquid after solution, and λ the quantity of heat thereby absorbed or evolved. The last λ is a general term compounded of 1, the heat become latent by the solution of the salt; 2, the heat disengaged by the change of volume; and 3, of the heat produced by chemical combination, when the salt undergoes such a combination.

Without considering how the one or the other of these Greek letters can be determined by itself, it is remarked, that their sum, positive or negative, is, in the first place, a *necessary* proportional of the mass of salt; and, secondly, is unchangeable, while the proportion of the salt to the water is not changed. When two experiments are made with *this proportion constant*, but the *temperature of the salt different*, the temperature of the water *besides* in both trials being either equal or not, we have in the first case,

* Poggendorff's Annalen, xxxv. 481.

$$M' (T'' - r') + m' c (t' - r') = m' \lambda,$$

$$\text{or, } M = \mu \, m'$$

$$\mu (T'' - r') + c (t' - r') = \lambda.$$

And in the last case

$$\mu (T''' - r') + c (t' - r'') = \lambda.$$

Eliminating λ from these two equations, we obtain the value of c, or the specific heat of the salt.

The following are the results:

Solutions of Common Salt.

Experiment.	Temperature of the Salt. °	Water. °	Solution. °	Weight of the Water. grms.	Salt. grms.	Salt in 100 water
1	15·29	1·0	13·95	76·595	5·955	7·75
	15·69	43·2	14·906	76·635	5·905	7·705
2	15·26	0·5	13·28	61·575	8·125	13·195
	15·06	43·6	14·07	64·700	8·100	12·983
3	15·914	0·5	13·047	80·540	25·540	31·711
	15·867	49·5	15·559	80·535	25·105	31·172
4	17·053	0·6	14·889	80·575	12·430	15·427
	17·267	45·3	16·296	80·570	12·385	15·372

Calculation gives the following values for c and λ.

Salt in 100 parts water.	c	λ
7·740	0·1725	15·002
13·089	0·1744	12·776
15·400	0·1781	11·483
31·441	0·1732	6·867

The mean value of c is $= 0·1743$. The value of λ changes with the quantity of salt. When the solution of the salt contains no more than 4 per cent. of salt, the value of $\lambda =$ 16·8. With a maximum of salt, its value appears to be $=$ 3·4. With a minimum $= 18·6$.

With a solution of sulphate of magnesia, the mean value of c was found to be $= 0·2906$ and λ a constant quantity.[*]

Temperature at different depths.—M. Quetelet has published some observations, which he has made at Brussels, in reference to the temperature at different depths. At a depth of 17 centimetres (6·18 inches), the mean temperature of the year 1834 was 51°; at 55 centimetres (21·6 inches), the mean temperature was 51°½; at 75 centimetres (29·47 inches), the temperature was 52°·19; at 1 metre (3·28 feet), 52·95.—(*Correspondance Mathem. et Phys.*, viii. 303.)

* Poggendorff's Ann. xxxv.

Mode of measuring high temperatures.—Lampadius, after endeavouring to point out the deficiencies in the modes which we already possess of ascertaining the melting points of those substance which require a high temperature for fusion, recommends his *Photoscope* for this purpose, which measures the light at different temperatures, such as dark red, cherry-red, red heat, light red, white heat and dazzling white heat. He gives, however, no description of his instrument, but refers to his *Beiträgen zur Atmosphärologie.**

CHEMISTRY.

I. *Solidification of carbonic acid.*—Thiloriet has succeeded in reducing this gas to a solid state, by exposing it to a temperature of 148° F. (?) Even when exposed to the air, it remains in this state for a short time. Its elastic force appears to be deteriorated by being solidified, as in this state it gradually dissipates. It may be also rendered solid by suddenly raising it from a liquid to a gaseous state. When a stream of the acid is directed into a small glass phial, the latter is filled with a white powder. If a small portion of the solid acid is placed in a stoppered vessel, it soon fills the flask with a thick vapour, and the stopper is forcibly expelled.—*Gazette Medicale, Oct.*, 1835.

II. *Naphthaline and its compounds.*—*Naphthaline* was procured by Laurent, by boiling coal-tar in the open air until it was deprived of its water, and then distilling it in a retort with a copper beak and a glass receiver. The first product is a yellow substance which turns black in the air, and deposits much naphthaline. The second contains more naphthaline; the third is viscid, orange-coloured, and contains much para-naphthaline. The last contains a substance with the colour of realgar, which has not been examined. The first oils produce the naphthaline. These are distilled and purified by crystallization by means of alcohol. The specific gravity of its vapour is 4·528 by experiment.

Hence, we may consider its composition,

10 atoms Carbon,	.	.	4·166 = 7·5	10 atoms
5 „ Hydrogen,	.		·347 = ·625	5 „
			4·514 8·125	

It is, therefore, a bi-penta-carbydrogen or C^{10} H^5.

* Journal für praktische Chemie, iv. 181.

1. *Chloride of naphthaline** of Laurent is obtained by combining chlorine with naphthaline without heat. It is a white powder, but may be obtained in rhomboidal plates by solution in ether. Smell strong. Melts at 284°. When distilled, it is decomposed, but it may be volatilized in an open tube. Insoluble in water; little soluble in alcohol; more soluble in ether. Boiling sulphuric and nitric acids decompose it. Potash takes up muriatic acid from it. Potassium destroys it. It consists of carbon 45, hydrogen 2·9, chlorine 52·1.

2. *Chloro-naphthalase.*—When chlorine begins to act upon naphthaline, an oil is formed which it is difficult to separate from the preceding chloride and naphthaline. By dissolving it in ether, and allowing it to stand for some hours, the latter separates. Lastly, by dissolving it in alcohol and allowing it to settle, we observe that the solid chloride precipitates first, then the oily chloride, and last of all, the naphthaline. In this way it may be isolated. It contains carbon 60·9, hydrogen 3·9, chlorine 35·2.

3. *Chloro-naphathalese.*—When naphthaline is treated with chlorine; after being liquified, the matter becomes solid. A product is obtained which affords chloro-naphthalese by the simple action of potash. The product is placed in a retort along with a strong solution of potash in alcohol. Heat is applied and the alcohol collected. Pour a little water on the residue, the excess of potash and some chloride of potassium will be separated. An oil is deposited which is treated again with alcohol and potash. It is then precipitated by water. In a few hours it becomes a pearly mass crystallizing by sublimation. This is chloro-naphthalese. It consists of carbon 61·4, hydrogen 3·, chlorine 35·6.

4. *Perchloro-naphthálese.*— If, instead of treating the preceding body with potash, we distil it, it is partly de-

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* Laurent employs a new nomenclature to designate this numerous class of compounds. It consists in changing the vowel of the final syllable of the name of the substance in proportion as the hydrogen is replaced by combining bodies. Chloro-naphthalase will contain 2 atoms of hydrogen less than naphthaline, and will have gained 2 atoms of chlorine. Chloro-naphthalese will contain 4 atoms of hydrogen less than naphthaline, and will have gained 2 atoms of chlorine. Chloro-naphthalise is not known. Chloro-naphthalose contains 8 atoms of hydrogen less than naphthaline.

composed, and a portion passes over with an oil. By expressing the product between paper we obtain a pure substance which crystallizes by means of alcohol in needles with a rhomboidal base. It is isomorphous with the preceding.

If this pyrogenous compound is treated with a current of dry chlorine at the usual temperature, the gas combines with it and forms a solid, which, when dissolved in ether, crystallizes in small prisms. It is colourless, insoluble in water, little soluble in alcohol, more so in ether. It may be distilled. It consists of carbon 25·4, hydrogen 1·2, chlorine 73·4.

5. *Chloro-naphthalose.*—When naphthaline is submitted to the action of chlorine it liquifies, and muriatic gas is evolved. The matter becomes solid. By applying heat and continuing the action, a crystalline mass is obtained, which may be purified by dissolving it several times in alcohol or ether. The crystals are oblique prisms. Chloro-naphthalose is white and insipid. It distils without change. Burns with a green flame. At a red heat, lime converts it into chloride of calcium and carbon. It consists of carbon 45·6, hydrogen 1·5, chlorine 52·9.

6. *Hydro-chlorate of chloro-naphthalase.*—This compound is produced by first passing a current of chlorine over naphthaline; this process should be stopped when the only product, which was heated during the re-action, begins to deposit a white matter. This oil is a mixture of naphthaline, oily chloride, and solid chloride. When exposed to a temperature between 122° and 140° in a small capsule, then dissolved in ether and exposed to a cold of 14°, the greater part of the solid chloride is deposited. The ethereal solution when mixed with alcohol and exposed to the air deposits ⅘ths of oil. The remainder, when exposed to a heat sufficient to expel the ether and alcohol, is pure hydrochlorate of chloro-naphthalase. It is only slightly yellow, soluble in alcohol and ether. Chlorine converts it into hydro-chlorate of chloro-naphthalese. It is decomposed by potassium, and partially by distillation. Its constituents are carbon 61·435, hydrogen 3·525, chlorine 35·040.

7. *Hydro-chlorate of chloro-naphthalese* or solid chloride is obtained by the process just described. After the action of

the chlorine has ceased, it is necessary to take up the oily matter with the ether, and to dissolve the residue in this liquid with heat in a closed flask, and to crystallize by cool-ing. Boiling sulphuric acid converts it, 1st, into a matter insoluble in water, and soluble in ether. When this solution is evaporated a transparent varnish is left. 2d. Another substance which remains in solution and gives, with barytes, an incrystallizable salt soluble in alcohol, which is probably a sulpho-salt analogous to the sulpho-naphthalates. Hydro-chlorate of chloro-naphthalese consists of carbon 44·79, hydrogen 2·70, chlorine 52·51.

8. *Bromo-naphthalase.*—When a few drops of bromine are poured upon naphthaline a lively action ensues, heat and hydrobromic acid are disengaged, and an oily product is formed. This consists of carbon 50·9, hydrogen 2·9, bromine 46·2. This oil is evidently a mixture of two substances, the first of which has not been separated, but the second.

9. *Bromo-naphthalese* may be obtained by distilling a mixture of bromine and naphthaline. Hydrobromic acid, a bromine oil, and charcoal come over, and towards the end of the process crystals of bromo-naphthalese appear. These are formed most completely when the bromine has been added in excess to the naphthaline. In dissolving this product in alcohol and evaporating, we obtain six-sided prismatic needles. They are white, insoluble in water, volatile, very soluble in alcohol and ether. They consist of carbon 42·9, hydrogen 2·1, bromine 55.

10. *Bromide of chloro-naphthalese* is formed by pouring bromine upon chloro-naphthalese in a close flask. The latter dissolves and solidifies into a crystalline mass. When purified by alcohol it resembles the chloride of chloro-naphthalese, and consists of carbon 23·5, hydrogen 1·05, chlorine and bromine 74·45.

11. *Nitro-naphthalase* is formed by the action of boiling nitric acid upon naphthaline. A new oil is obtained first, which solidifies very slowly by cooling, forming a crystal-line mass of large needles. It consists of two bodies very soluble in alcohol and ether, the one is solid or nitro-naph-thalase, the other is liquid. The former is expressed be-tween folds of paper. It is then dissolved in alcohol. On

cooling, drops subside to the bottom of the vessel, containing much nitro-naphthalase, which is separated by solution in alcohol. The alcohol lets fall crystals. They are four-sided prisms terminated by acute pyramids. Colour sulphur-yellow. Volatile. Insoluble in water; very soluble in alcohol and ether. Analysis gave carbon 69·86, hydrogen 4·07, azote 8·53, oxygen 17·54.

12. *Nitro-naphthalese* may be formed by boiling the preceding with nitric acid for a long time. An oily layer appears. The whole is evaporated rapidly. When the oil and acid are nearly equal in quantity, the two layers become confused, and if the vessel is removed from the fire, the whole becomes solid which is nitro-naphthalese. It presents the form of microscopical needles. It is neutral, insoluble in water, very little soluble in boiling alcohol. It contains carbon 54·83, hydrogen 2·9, oxygen 29·57, azote 12·7.

13. *Naphthalase.*—If we heat nitro-naphthalase with from 8 to 10 times its weight of lime in a small retort filled up to the neck, a brown oil is disengaged containing naphthaline, ammonia and undecomposed nitro-naphthalase, while a thick oil condenses in the neck of the retort which becomes solid on cooling. The lime is blackened by a deposition of charcoal. The solid matter is washed with ether which takes up the foreign matter. A yellow powder naphthalase remains. Cold sulphuric acid forms with it a fine blue solution. It resembles *Idrialine* in this property. It consists of carbon 87·, Hydrogen 4·8, oxygen 8·2.*

III. *Compounds of phosphorus and hydrogen.*—M. Leverrier finds that when phosphuretted hydrogen is exposed to the action of light, the sides of the glass are speedily covered with a yellow amorphous matter, which may be dried and heated at a temperature of 284°, without either undergoing combustion or becoming luminous. It is insoluble in water and alcohol; chlorine changes it into hydro-chloric acid and chloride of phosphorus; nitric acid causes it to burn. With solutions of copper, silver, &c. it gives phosphurets containing more phosphorus than those obtained by the phosphuretted gas. It consists of hydrogen 1623, phosphorus 195 or H$\frac{1}{2}$ Ph. It may be also prepared by

* Ann. de Chim., lix. 196.   Journ. de Chim. Medic., i.

passing chlorine through phosphuretted hydrogen. The gas which remains after the separation of the sub-hydret of phosphorus is a sesqui-hydret consisting of 18·719 hydrogen and 201·11 phosphorus or $H_{1\frac{1}{2}}$ Ph. Leverrier considers that the sub-hydret is formed by the light, and does not exist in the inflammable gas; because as analysis always indicates an excess of phosphorus in the latter, we cannot consider it a pure chemical compound, but rather a mixture of sesqui-hydret ($H_{1\frac{1}{2}}$ Ph) with a compound containing less hydrogen, which cannot be the subhydret ($H_{\frac{1}{2}}$ Ph) as this is solid. He conceives that it must be a compound H Ph, which has not yet been isolated, corresponding to hypo-phosphorus acid, and inflaming in the air. When exposed to the action of light it is decomposed into $H_{\frac{1}{2}}$ Ph and $H_{1\frac{1}{2}}$ Ph. and is, therefore, a compound possessing little stability like nitric oxide to which it corresponds. He estimates the quantity of sub-hydret at $\frac{2}{30}$, and the hydret or H Ph at $\frac{1}{15}$ of the weight of inflammable gas. The gas prepared from phosphorus acid consists of ·1872 hydrogen, and 1·966 phosphorus $= H_{1\frac{1}{2}}$ Ph, and is, therefore, the same as that produced by the action of light upon inflammable gas.

This is certainly a very simple view of the subject.

Leverrier, like his countrymen on other subjects, seems ignorant of what has been done before him to clear up the anomalies in the combinations of phosphorus and hydrogen. If he had made himself acquainted with the ingenious researches of Mr. Graham, he would have discovered that his theory does not agree with the experiments of Mr. Graham, for the latter found that potassium and other substances, in very minute quantity, destroyed the inflammability. If, however, we suppose that Mr. Graham performed his experiments under the influence of circumstances calculated to produce the decompositions which Leverrier has shewn to take place, then the anomalies are removed.

The compounds of phosphorus and hydrogen, according to Leverrier, therefore, are

1 Sub-hydret. $H_{\frac{1}{2}}$ Ph Solid.

2 Hydret . . H Ph A gas spontaneously inflammable in the air, which has not yet been isolated; decomposed by light.

3 Sesqui-hydret   H¹¹ Ph   A gas not inflammable; decom-
                                              posed by light.
4 Perhydret   .   H²¹ Ph   A gas produed by heat from
                                              phosphite of lead according to
                                              H. Rose.*

IV. *New class of Borates.*—In a paper published in the *Kongl. Vetensk. Acad. Handl.*, for 1834, a class of simple borates is described. Borax and carbonate of soda were boiled together. An effervescence took place; the gas which was disengaged was passed through a tube containing lime-water; a precipitate fell. Hence, the opinion, that borax is a biborate appears correct. Crystallized *borax* and an-hydrous *carbonate of soda* were mixed and heated to the melting point of silver, in a platinum crucible; the mass had lost all the water of the borax and the carbonic acid of the soda, but acquired no appearance of fusion. It was dissolved in water, concentrated, and allowed to crystallize in an air tight vessel. Sharp four-sided prisms appeared with truncated extremities. The angles were 70° and 110°. This salt has an alkaline taste, and absorbs carbonic acid from the air. It melts at 57 C (134°·6 F.) in its water of crystallization, but does not crystallize on cooling. A portion of the salt melted in its water of crystallization remained for many days at the temperature of 32° without crystallizing.

1·046 grms. of the original crystals of this salt lost by heating 0·502 grm. of anhydrous salt, and lost 0·542 water, which the author considers equivalent to 8 atoms of water and the composition of the salt $NaB + 8\ HO$.

4·098 grms of the crystallized and melted salt, gave 2·26 after heating = 6 atoms of water. 8 atoms are equal to 52·11 per cent. and 6 atoms to 44·832 per cent. of water.

A similar salt is obtained by heating in a white heat boracic acid and carbonate of potash. It dissolves in very little water, and it is, therefore, difficult to obtain it in regular crystals. Solutions of these salts added to neutral solutions of earthy and metallic oxides, precipitate borates of the same composition.—(*Poggendorff's Ann.* xxxiv. 561.)

V. *Xanthic acid.*—This acid may be prepared from the Xanthate of potash by means of sulphuric acid. The Xan-

* Ann. de Chim. lx. 174.

thate is formed by neutralizing an alcoholic solution of potash with bi-sulphuret of carbon. Zeise has analyzed the acid and finds it to consist of

Sulphur	.	56·440 = 8·	. . 4 atoms.
Carbon	. .	32·169 = 4·5	. . 6 ,,
Hydrogen	.	4·377 = ·625	. . 5 ,,
Oxygen	.	7·014 = 1·	. . 1 ,,

$$\overline{\quad 100· \quad\quad 14·125 \quad}$$

Its formula is, therefore, $S^4 C^6 H^5 O$.

				Xanthic acid.
Xanthate of potash contains	potash	. . .	29·244	70·756
Xanthate of soda ,,	soda	. . .	21·536	78·464
Xanthate of barytes ,,	barytes	. .	40·402	59·598
Xanthate of lead ,,	oxide of lead		49·638	50·362
Xanthate of copper ,,	oxide of copper	38·0		62·0

(*Poggendorff's Ann.* xxxv. 487.)

VI. *Bromide of deuto-carbydrogen.*—Regnault formed this compound which was previously known, by dropping bromine into a current of olefiant gas. It is a colourless liquid, with a sweet taste. Density 2·164. It consists of

2 vols. carbon	.	·8333	. 1·5
2 ,, hydrogen	.	·1398	. ·25
1 ,, bromine	.	5·5555	. 10·

$$\overline{\quad 6·5286 \quad\quad 11·75 \quad}$$

The density of the vapour by experiment was 6·485. Its formula is $C^2 H^2 Br$.

Iodide of deuto-carbydrogen.—The best method of obtaining this compound, is to pass olefiant gas into the bottom of a matrass with a long neck containing iodine, at the temperature of 131°. The iodine speedily melts, and yellow needles appear in the neck of the vessel, which become white by the action of the gas. In taking them up by water holding in solution some ammonia, a crystalline substance is obtained, which when washed and dried in vacuum, constitutes iodide of deuto-carbydrogen. By drying, it always becomes slightly yellow. It melts at 163½°. Insoluble in water. Soluble in ether and alcohol. It decomposes so easily that it is difficult to determine the density of its vapour, but analysis shewed its composition to be,

2 vols. carbon . . .	·8333	.	1·5
2 vols. hydrogen . .	·1398	.	·25
1 vol. iodine . . .	8·6780	.	15·75
	9·6511		17·50

Its formula is $C^2 H^2 I$.

Bromide of aldehydene.—If we mix some bromide of deuto-carbydrogen with a concentrated solution of caustic potash in alcohol, a white precipitate subsides, the liquor effervesces, evolving a peculiar odour. If the mixture is kept at the temperature of about 95°, a gas distils over having an odour of garlic. This is bromide of aldehydene. It may be purified by passing it through water and chloride of calcium. The density of its vapour is 3·691. Analysis gave its constituents, carbon 22·674, hydrogen 2·923, bromine 74·603. It appears, therefore, to contain just half the quantity of bromine which the bromide of deuto-carbydrogen possesses. I should, therefore, be inclined to consider its formula $C^2 H^2 Br^{\frac{1}{2}}$, notwithstanding the new name which Regnault has given it, and the theoretic views which he has propounded. It is, therefore, a sub-bromide of deuto-carbydrogen.

Iodide of aldhydene is formed in the same way as the last compound. A gas comes over with an odour of garlic. When exposed to the cold of a mixture of ice and marine salt, a portion is condensed which is the iodide. The density of its vapour is 4·78. This corresponds nearly with

2 volumes carbon, .	·8333	1·5
2 volumes hydrogen,	·1398	·25
½ volume iodine, .	4·3390	7·875
	5·3122	9·625

This is also a sub-bromide of deuto-carbydrogen.—(*Ann. de Chim.*, lix. 358.)

M. Darcet Fils has also examined the first two compounds. The results agree with those of Regnault.—(*Journ. de Chim. Medic.*, i. 377.

VII. *Composition of the Dutch liquor.*—Regnault has examined this substance after taking every precaution to purify it, and has found its composition the same as that obtained by Dumas, viz.

Chlorine, . . . 71·32
Carbon, ... 24·65
Hydrogen, . . . 4·03
_____
100·00

Its specific gravity he found 1·256. Its boiling point 180°½. The density of its vapour was 3·45 by experiment.

VIII. *New ether.*—When a solution of caustic potash in alcohol is agitated in the Dutch liquor, white crystals are formed, and if after digestion for some hours, the hand is applied to the vessel in which the mixture is contained, bubbles are disengaged which burn with a yellow flame, with green edges resembling hydro-chloric ether. It is composed of

Chlorine, . . . 57·08
Carbon, ... 38·09
Hydrogen, . . . 4·83
_____
100·00

Regnault conceives that the Dutch liquor being a compound of one atom of each of its three elements is composed of this new ether and muriatic acid. The new ether liquifies at about — 17 C (+ 1° F.) It possesses an odour like garlic, and is destroyed by the electric spark. When heated with potassium, carbon is deposited and a white vapour forms, which is probably naphthaline.—*(Ann. de Chim.*, lviii. 301.)

IX. *Action of Diatase.*—Diatase prepared in the manner already described (*Records,* vol. i. p. 196.) possesses the following properties as determined by Guerin. 1st. One part of diatase dissolved in 32 of water and mixed with 4·08 starch produced no change in 63 days. 2d. The three parts of starch added to two of diatase do not increase in size at a temperature below 129°, at which water causes it to burst. 3d. Diatase liquifies and converts into sugar, starch previously made into a paste without any absorption or disengagement of gas, both in air and in vacuo. 4th. 100 parts of starch converted into paste with 3900 parts of water, treated with 6·13 parts of diatase dissolved in 40 parts of cold water, and kept during an hour between the temperatures of 140° and 149°, gave 86·91 parts of sugar. 5th. A paste of 100 starch and 1393 water brought in con-

tact with 12·25 parts of diatase dissolved in 367 parts of cold water, when preserved at a temperature of 68° for 24 hours, produced 77·64 parts of sugar. 6th. The same experiment repeated at the temperature of melting ice gave, at the end of two hours, 11·82 sugar. 7th. Between 10° and 23° starch paste is rendered fluid without the production of sugar. 8th. The most favourable proportions and circumstances for the production of a great quantity of sugar are a slight excess of a diatase or sprouted barley, about 50 parts of water to one of starch, and a temperature between 140° and 149°. 9th. Starch-sugar prepared either with diatase or sulphuric acid crystallizes in cauliflower like forms, or in prisms with rhomboidal faces. It has the same composition as grape-sugar. 10th. Diatase even in excess does not convert into sugar gummy matter dissolved in water with starch-sugar, but when this matter is isolated it converts it almost completely into sugar. 11th. Diatase produces no effect on gum arabic, cane sugar, nor sugar of yest. 12th. A solution of diatase in water decomposes in the air. 13th. When sugar of starch, obtained either by sulphuric acid or diatase, is submitted to the spirituous fermentation, the sum of the weight of the alcohol, carbonic acid and water of crystallization differs from the weight of the sugar by about three and a half hundreths, proceeding from the formation of acetic and lactic acids, &c. 14th. To determine as exactly as possible the quantity of alcohol in a liquid containing a substance which retains the alcohol strongly, it is necessary to push the distillation until the liquid passing over no longer affects the centesimal areometer.—*(Journ. de Chim. Medic.*, i.)

X. *Method of colouring ornaments of gold.*—Manufacturers possess a number of receipts for colouring ornaments, but the following is most commonly employed: 2 parts of salt-petre are mixed with 1 part of sea-salt and 1 part of Roman alum, in a quantity of this mixture equivalent to about three times the weight of the ornaments to be coloured, dissolved in boiling water so as form a very concentrated solution where the ornaments are placed. This solution is called the *sauce.* Here they remain at a boiling temperature for 15 or 25 minutes, according to the shade to be given them; they are then washed in pure water and the operation terminates. If lustre is required, they are after-

wards burnished; their weight is diminished about $\frac{1}{16}$. The *sauce* takes up some copper, silver, and a certain quantity of gold; it is preserved for the purpose of extracting these metals. After it has been used, it takes the name of *colour water*. When allowed to stand at rest, it becomes limped, and a white deposit separates, called *deposit of the colour waters*, and the supernatant liquor is termed *clear waters*. Sulphate of iron is added to the clear waters, and then bars of iron are plunged into them. A precipitate containing gold falls down, called *black matters*. The white deposit, consists of water 10·8; soluble salts 48·8; insoluble matter 39·8 = 99·4. The insoluble portion contains, sub-alum 71·8; proto-chloride of copper 5·0; chloride of silver 8·5; oxide of iron 14·; metallic gold ·776 = 100·076. The black matters consist of, water 13·1; soluble salts 44·5; insoluble matter 41·8. The insoluble matter contained oxide of iron 64·; oxide of copper 26·; metallic gold 5·08; metallic silver 1·12 = 96·2. The assayers fuse the black matters with a mixture of potash, pearlash and borax to extract the gold and silver. The composition of the deposit from the colour waters, shews that in the action which the mixture of salts exercises upon the alloy plunged in the boiling sauce, the alum is decomposed, and abandons sulphate of potash and a great part of its sulphuric acid, to be transformed into a double insoluble sub-salt. The sulphuric acid which the potash loses, is taken up by the potash of the nitre, and by the sodium of the sea-salt, converted into soda by the agency of the nitric acid set at liberty.

By the process described then, the concentrated colour-water dissolves a portion of the gold at the temperature of ebullition; the metal remains in the liquid in the state of chloride, and a deposit of sub-alum takes place. The silver is still more strongly attached and is converted into chloride, and if the proportion of marine salt is sufficient, this chloride dissolves like the gold, but on cooling, a portion separates, and if the liquor is much diluted with water, the remainder precipitates and the solution only retains slight traces.*

Pyruric acid.—This acid was obtained by distilling tar-

* Ann. de Chim. lx.

taric or racemic acids at the temperature of 392° F., and rectifying in the water bath, the product of the distillation, which is a yellow matter. In this second process, the first half which contains acetic acid is laid aside. The last half is a yellow liquid with a feeble smell, a thick consistence and an acid taste, specific gravity 1·25. It does not crystallize at 41°. It consists of carbon 46·042; hydrogen 3·762; oxygen 50·195.

It appears, therefore, to be tartaric acid, combined with the half of its radicle, or pyro-tartaric acid, combined with an atom of carbonic acid. Its atomic weight is 9·961, and its capacity of saturation 10·04. Its salts do not crystallize but present an appearance like gum.—(*Journ. de Pharm.* May, 1835.)

When citric acid is distilled at a temperature of between 392° and 482°, an acid is obtained distinct from pyro-citric acid. It differs only from citric acid in having an atom less water, and the same capacity of saturation.

Nitro-sulphuric acid.—M. Pelouze formed this acid by causing the deutoxide of azote to act upon a solution of sulphite of potash and potash dissolved in water; the quantity of gas absorbed is in the proportion of 2 vols. to 1 of sulphurous acid contained in the salt. Now, 2 vols. sulphurous acid, and 4 vols. deutoxide of azote, combine with the alkali and form a new compound, the formula for which is, $Az^2 SO^4 + KO$.

This salt crystallizes in beautiful quadrangular prisms, but is decomposed in contact with water at the temperature of 32°, giving origin to sulphate of potash and protoxide of azote. All the acids, even the most weak, produce a disengagement of protoxide of azote, but when the heat is gentle, sulphate of potash is formed, and deutoxide of azote disengaged. Nitro-sulphate of ammonia in decomposing, sometimes gives out so much heat as to produce explosions, by decomposing the sulphate of ammonia formed.

This salt is decomposed by the bodies which have the same effect on deutoxide of hydrogen; this phenomenon takes place even with bodies which do not decompose the latter. It affords a ready method of disengaging protoxide of azote in the stomach. Majendie is trying its effect as a remedial agent.—(*Journ. de Chim. Medic.* i. 438.)

ARTICLE VII.

ANALYSES OF BOOKS.

Proceedings of the Berwickshire Naturalists' Club. Part III.

THIS Number contains, 1. An address by the President. 2. A description of *Natica helicoides*, a new British shell. By Dr. George Johnston. The characters are, *Shell* ovato-conical, smooth, white, immaculate, covered with a yellowish epidermis; whorls 5, rounded, separated by a channelled suture, the spire produced and rather obtuse; aperture pure white, with a small fissure on the pillar. Length six-tenths; breadth scarcely four-tenths. *Hab.* Berwick Bay. *Obs.* This new species was found in the refuse of a fishing-boat. When the epidermis is removed, the whorls appear to be finely striolate in a spiral direction. Animal unknown.

3. List of the *Malacostraca Podophthalma* found on the coasts of Berwickshire and North Durham. By Mr. R. Embleton, Surgeon. In this list the author describes a new species of *Galathea* to which he has given the specific name *nexa.* Its characters are arms hirsute, large; the hand without spines, the wrist with a single one on the inner side, or, when two, the anterior is much the smaller; ligament of the shell brown. Three specimens of this hitherto unobserved species have only been found, two in Berwick bay, and the other in Embleton bay.

4. Contributions to the Flora of Berwickshire.

5. Remarks on the mode of formation of the "Fairy Stones" found near Melrose. By the Rev. A. Baird. A description and figure of these have been given in *Records*, vol. ii. 1. The author supposes that they are stalactites. This is, however, impossible, because they consist essentially of an insoluble, mechanically formed rock. They most likely have been produced by the action of the water at the bottom of a fall, where round basins, and a variety of figures may often be observed. An examination of the locality would determine the origin.

6. A catalogue of the Bivalved Shells found on the coast of Berwickshire and North Durham. By Dr. Johnston. These amount to 70. The rarest are *Pecten lineatus, P. spinosus, Lima fragilis, Arca fusca, Kellia rubra, Anatina pubescens, Tellina crassa, Psammobia florida, Astarte compressa* and *Mya norvegica.* The cockle and mussel are common. The clams (*Pectinidæ*) are rare, except the small obsoletus which.is the favourite food of the flounder. The only oyster-bed is in the channel between Holy Island and the main land, and is the private property of the Earl of Tankerville. In the inventory of the Priory of Holy Island for 1381-2, there is an item of expenditure for "a sloop (*navicula*) bought of a certain Scotchman (*de quodam skoto*) with the oysters and other goods contained in it, 100s."

7. Catalogue of Insects found at Berwick-upon-Tweed, in August, 1834. By Charles C. Babington.

8. Notice of the *Parr.* By Sir William Jardine, Bart.—(See *Records*, vol. iii. p. 269.)

9. On the Instinct of the Water-Hen. By P. J. Selby, Esq.

10. Observations on the Strata of Berwickshire and North Durham. By Robert D. Thomson, M. D.

These relate to the determination of the age of the strata on the Tweed, which have been usually assigned to the New Red Sandstone formation. The rock to which this appellation has 'been given lies over magnesian limestone in several places. A section is given of the strata of the Durham coal-beds down to the seam which is at present worked, at a depth of 31 fathoms. A fossil tooth is also described and figured.

11. Notice of the Skeleton of a Red Deer (*Cervus Elaphus*) found at Cheswick, North Durham. By J. S. Donaldson, Esq., of Cheswick.

12. Remarks on the Tumulus at Cheswick. By J. S. Donaldson, Esq. In this tomb the remains of a skeleton were observed, and the head of a brass spear which must have been originally highly polished, but now covered with verdigris. The tumulus was 20 feet high, and the area of its base 50 feet in diameter.

13. Contributions to the Entomology of Berwickshire. By P. J. Selby, Esq. and Dr. Johnston.

14. List of the Entomostraca found in Berwickshire. By Mr. William Baird, Surgeon.

In addition to well known species of this remarkable tribe of animals comprehending *Cyclops staphylinus, Cyclops rubens, C. lacinulatus, C. vulgaris, C. minuticornis C. brevicornis, Cythere flavida, C. gibbera, Cypris detecta, C. striyata, C. vidua, C. pubera, C. monacha, C. reniformis, Daphnia quadrangula, D. pulex, D. sima, Lynceus sphæricus, L. quadrangularis, L. lamellatus, L. trigonellus, L. truncatus,* the author describes fifteen new species, whose characters we shall give for the benefit of those who interest themselves in such researches.

1. *Cyclops Johnstoni.* Nova species. Pools of sea-water at Berwick and Cockburnspath. Body of four segments, tail of six, terminated by two short lobes, from which issue two long setæ, fully the length of the body. Superior antennæ of about six articulations, stronger than inferior pair. In the male there is a bulla about fifth articulation. In the female they are more slender, more setiferous, and destitute of bullæ. Inferior antennæ of three or four articulations; terminated by two or three short setæ. All four antennæ setiferous at base of articulations. Head beaked. Beneath the antennæ are two organs (palpi?) of two articulations, setiferous at base of articulations and at extremities. Beneath these are two organs, which Muller calls hands, of two articulations, terminated by a strong curved moveable claw or hook; and beneath these again, are two double organs, or *membres particulieres* of the French authors, each pair consisting of a short strong common footstalk, from which arise two flat bodies, the superior of which is the longer, of two articulations, serrated above, and terminated by three short setæ ; the inferior, also serrated above, and terminated by three setæ, but consisting of only one articulation. From the three inferior articulations of the body arise three pairs of long setiferous feet ; and

from the second articulation of the tail arise the sexual organs in either sex. This species approaches the *C. chelifer* of Muller, but differs in many points when closely examined. In Muller's species there are no articulations to the body, which gradually tapers to the tail, and which he describes as *" farciminis facie."* The superior antennæ are only of three articulations; the inferior, which he calls *" palpi,"* of two. The organs beneath these, which I call *" palpi,"* are furnished with a claw, and only of one articulation, whilst the last pair of particular members, have only the shorter of the two bodies of which they are composed, serrated, the longer being entire. The male in Muller's figure has not the bullæ on antennæ.

2. *Cythere reniformis.* Nova species. Sea-shore at Berwick and Eyemouth, &c. Shell reniform; flesh-coloured; covered with hairs; both extremities of equal size; antennæ furnished with numerous short setæ to all articulations; anterior feet falcate, entire; all the feet furnished with claws. This species approaches the *C. viridis* of Muller, but differs in colour, in both extremities of shell being equal, and in anterior feet not being serrulated. It differs from *C. lutea* in shell being covered with hairs.

3. *C. alba.* Nova species. Shore near Dunbar. Shell white, transparent, hairy, acute at posterior extremity, and broader at anterior; a rim round edge of shell whiter than the rest of shell; antennæ beset with short setæ at each articulation.

4. *C. variabilis.* Nova species. Shore at Cockburnspath and Eyemouth. Shell glaucous, without hairs, ovate, anterior narrower than the posterior extremity; anterior legs falcate, and furnished with pretty strong claws; antennæ slender, without setæ. This species varies much in colour and markings. Some specimens are white, with two black fasciæ, one at posterior margin, and the other across centre of shell, while the posterior extremity is marked besides by a beautiful reddish or bright bronze spot; other specimens are of a light flesh colour, with the edges of shell slightly greenish, and the body of the shell marked with dark streaks running across. Some are altogether of a fine flesh colour; while others again are of a very dark brown. All the varieties, however, agree in shape of shell, in size, &c., merely differing in colour and marking. Future observations may perhaps determine them to be of two different species.

5. *Cypris Joanna.* Nova species. Pool near Abbey St. Bathans. Shell roundish, ovate, narrower anteriorly than posteriorly; of a brown colour, with an orange mark across back of shell and lower margin; shell beset all round with rigid hairs, and covered with minute black points or dots; setæ of antennæ numerous, about twelve or more. Resembles *C. vidua* a good deal in shape, but differs totally in colour and markings. Differs from *C. pilosa* somewhat in shape, and in not being glabrous, but marked all over with black roughish-looking points.

6. *C. minuta.* Nova species. Pool on Beaumout water at Yet-holm. Shell broader posteriorly than anteriorly; elevated and rounded on upper margin; slightly sinuated on under margin; hairy all round; of a light brown colour with a tinge of green; body of

shell smooth, shining; posterior legs terminated by one long claw; anterior legs furnished with a pencil of long hairs from penultimate joint, and terminated by several strong hairs or setæ; setæ of antennæ numerous.

7. *C. elongata.* Nova species. Pool on Beaumont Water at Yetholm. Shell much broader at anterior than posterior extremity, which is narrow and much elongated; elevated on upper margin towards anterior extremity, and situated on under margin more towards the posterior extremity; white; transparent; hairy; setæ of antennæ five or six; anterior feet of about three articulations, each articulation furnished with setæ; posterior legs denticulated.

8. *C. reptans.* Nova species. Yetholm Loch. Shell long almost elliptical, nearly plane on upper, and slightly hollowed out or sinuated on under margin, rather ventricose, hairy; densely ciliated on anterior extremity; the ciliæ on posterior extremity fewer but much longer, of a light colour with dark green markings, which appear to be rather irregular; both extremities have a large broad green spot, which sends out processes as it were towards the centre of the shell; antennæ and feet short in comparison to the size of shell. I have never seen this species swimming about in the vessel in which I have kept it, but always creeping on the bottom.

9. *C. Westwoodii.* Nova species. Yetholm Loch. Shell much elevated and rounded on upper margin, and sinuated on lower, broader at anterior extremity, green-coloured, semi-transparent, densely covered with pretty long hairs all over; second last joint of anterior feet furnished with a pencil of long hairs; posterior feet furnished with a short seta at each articulation, and with a long curved claw at extremities; antennæ indistinctly articulated.

10. *C. tristriata.* Nova species. Pond at little Swinton. Shell ovate, ventricose, anterior extremity a little narrower than posterior, upper margin rounded, lower sinuated slightly, green, hairy; on the upper margin, nearly in middle of length of shell, there is a dark mark, from which run to posterior extremity three dark green streaks, the centre one of which is the most distinct and the darkest coloured; anterior extremity of a rather darker green than the rest of shell. Between the centre and most anterior of the streaks are five or six small lucid spots.

11. *C. hispida.* Nova species. Pool on Beaumont Water at Yetholm. Shell almost elliptical; the anterior extremity being a little broader than posterior; rather ventricose; very roughly and densely hairy; of a brown colour all over, with one or two dark brown marks running across centre of shell, in the anterior of which are four or five translucent spots; both extremities of a darker colour than other parts of shell. The whole shell is very hispid, spines rather than hairs covering the shell; antennæ slender; setæ seldom much divaricated, about twelve in number. The markings of shell are not in all specimens very distinct.

12. *C. lucens.* Nova species. Yetholm Loch and pools on Beaumont Water. Shell white, shining, without spot; almost opaque; ventricose; elevated on upper margin towards posterior extremity, and reniform underneath; anterior extremity narrower and flatter than posterior which is arched and broad, the inferior angle being, however,

prolonged to a point ; a few fine hairs at each extremity. This species differs from *C. detecta* in being ventricose, and more arched in upper margin ; and from *C. candida* in being reniform, in not being ovate, and in want of rigid hairs which beset that species.

13. *C. compressa.* Nova species. Yetholm Loch. Shell round, shaped, compressed rather narrower anteriorly than posteriorly ; of a grey colour, more or less deep ; semi-transparent ; at either extremity beset with fine hairs ; in some specimens spotted as if little pieces were hollowed out ; anterior feet provided with several long bristle ; eye large ; antennæ terminated by numerous long setæ. From the flat compressed shape of shell, its motion through the water is very much like that of some species of Lynceus.

14. *Lynceus harpæ.* Nova species. Pool on Beaumont Water, and in Dunglass Pond. Shell harp-shaped ; ribbed longitudinally, the ribs resembling the strings of the harp ; rounded posteriorly, sinuated anteriorly, and terminating in a point projecting forwards ; antennæ four, long, nearly the length of the shell, each consisting of three articulations, and terminated by three long linear setæ ; shell smooth, except anterior edge where it is sinuated, being there ciliated ; tail serrated, terminated by two strong setæ ; head rounded, and beak blunt. Differs from *L. truncatus* in sinuated anterior margin of shell, blunt beak, and long antennæ ;· in not being truncated on posterior extremity ; in wanting the thirteen little teeth at the base ; and in wanting the two thick and large upper feet : differs from *L. quadrangularis* in shape, in sinuated anterior margin, in more distinct ribs, and in blunt beak.

15. *L. hamatus.* Nova species. Yetholm Loch. Shell truncated anteriorly, and ciliated ; upper part gibbous ; tail not serrated, gibbous, terminated by two setæ ; two upper feet large, and each furnished at extremity with a strong claw or hook curved upwards ; antennæ of three setæ each : approaches *L. trigonellus*, but differs from it in beak being blunted and stronger ; in tail not being serrated ; in wanting the strong pediform organ below palpi and above the feet ; and in the upper feet having the strong hooks.

The descriptions of the Entomostraca, and those of the new shell *Galathea nexa* and fossil tooth are illustrated by three plates of etchings, beautifully executed (we must tell it) by an accomplished female member of the club.

The results of the labours of this association, during the past year, are thus shewn to be satisfactory in the highest degree. We hope its members will persevere. They have instituted an admirable school for themselves, and they must effect a great deal if they continue to persevere. We understand that similar clubs are now forming upon the same plan in various counties. We wish them every success.

Article VIII.

SCIENTIFIC INTELLIGENCE, &c.

I.—*Adulteration of Jalap Roots.*

A very clumsy adulteration of this article was detected by Herberger. Portions of the root were joined together with flour, and

covered on the outside with tincture of jalap.—*Buchner's Repert.,* xlviii. 118.

II.—*African Guaiac.*

THE negroes employ in syphilis, instead of guaiac, the hard and veiny wood of a shrub belonging to the tribe of Leguminosæ, which was described by Linneus under the name of *Guaiacum Afrum,* Sp. 547. The plant was, however, little known. It was afterwards described by the title of *Theodora speciosa.* More lately it was placed under *Cassia,* and by Jacquin it was termed *Scotra speciosa* from the appearance of its flowers, (*Icon rar,* i. 75.) and also by Andrew, (*Bot. Report,* 345.) It is little used in Europe, although it is much milder in its action than the common guaiac wood.— *Brandes' Pharm. Zeitung,* No. xxiv., 1835.

III.—*Progress of Geographical Discovery.*

CAPTAIN MACONOCHIE gave an interesting summary of the progress of Geographical Discovery on the 3d of February, at the London University, and similar to that of which we inserted a report last year.—(*Records,* vol. i. p. 155.) He began with

1. *North America.*—The object for which Captain Back's expedition was designed, it is well known, was one chiefly of humanity; it was for the purpose of gaining tidings of the party of Captain Ross. Without requiring, however, to direct his energies on this object in consequence [of timely information of the safety of Captain Ross, Captain Back has accomplished much in reference to completing the geography of North America. He first proceeded to Great Slave Lake, which he found to be one of the longest lakes in America. From thence, he ascended by a river which discharges its waters into the lake; and having transferred his stores over a portage, he arrived at a large river, which he descended to the sea, first north-east, then east, and lastly, in a north-east direction. His journey terminated in N. L. 67°·7, W. L. 94°, very nearly south south-west of Ross's Isthmus of Boothia, and about 90 miles to the south of it. From this, he attempted to get to the westward, but was prevented by the quantity of ice which was thrown up.

From his observations, it would appear, that Boothia is an island with a clear sea to the south of it. Captain James Ross found the ice changed to the south of Boothia in the course of one season, which would indicate the existence of a current and a free passage. The opposite shores of the land of Boothia, and that where Back's river terminates, are different in regard to geological structure, the former, being lime-stone (primitive lime-stone?) and the latter, granite. There are still, therefore, two questions for solution; Does the river of Back fall into the Northern Ocean, or into the bottom of Prince Regent's inlet? and, Where is the north-east point of North America? Most of those who have been connected with the arctic expedition, conceive that there is a passage by Melville island to the westward, and recommend determining the question at this point. In order to complete the geography of the coast, from Point Turn-

again to the eastward, Captain Back recommends sending a vessel
with stores to Wager's inlet supplied with four portable boats. Mr.
King, the companion of Back, in his last expedition, considers, that
a better, and much less expensive plan, would be, to make for Atha-
pescow lake; and thence, ascend a river, which, according to the
Indians, falls into the lake from the north; a portage of a few miles
conducts to the banks of another river, which terminates in Back's
river. The plan, however, advised by Captain Back, is one, which
appears to be most feasible, and is likely to be soon carried into exe-
cution. A third method which has been pointed out, is, to proceed
by Bathurst Inlet and Point Turnagain.

Besides this line of coast, which requires to be completed in a
geographical point of view, there are still 120 miles untraversed
towards Bherings Straits, uninteresting it is true, except in so far
as there appears to be a large river which discharges itself into the
sea at some point of this line of coast.

2. The lecturer described the enterprising journey of Lieutenant
Smythe from Lima, across the Andes, down one of the tributaries of
the Amazon to the mouth of the latter river, a distance of 2000 miles.
That traveller is now preparing for the press an account of his expe-
dition. He describes the inhabitants of Bolivia, as well behaved,
quiet, although, rather given to intoxication, and carrying on the
tillage of the land with spirit. There are few Spaniards in Lower
Bolivia, the inhabitants being all indigenous. Humboldt states, that
if the waters of the ocean were to rise 1000 feet, this portion, and,
indeed, the whole of the central part of South America would be
submersed.

3. *Africa.*—In consequence of the violent disturbances on the
Caffre frontier, and the consequent unsettled state of the country,
the journey of Captain Alexander has been postponed for a year.

But, although, the expedition under the direction of the geogra-
phical society has thus been stationary, much has been effected by
Dr. Smith, whose journey we formerly noticed, and from whose
labours we had great anticipations; we have not been disappointed.
He was enabled to fit out his expedition, entirely by a private sub-
scription of £3000, liberally supported at the Cape, and assisted in
this country. He proceeded first to Phillipolis, but was obliged, in
consequence of the Caffre war, to return. He then proceeded to-
ward Kurrechane, (about Lat. 24°,) and having been kindly received
by the powerful and intelligent king of that country; he was enabled
to reach that town and to advance beyond it. He proceeded, follow-
ing the river to the north-east, which appeared to bend towards
Delagoa bay. He ascended a high ridge of mountains and had a
delightful prospect, the coast being apparently within 60 miles. He
visited, likewise, a very large lake, which is so broad, that the shores
are lost sight of in crossing it. The boats here are built, and not
hollowed out of trunks of trees. The only information which has
yet reached this country of the expedition, has been through the
Cape papers, and some letters written by Dr. Smith, to a friend, in
London. We may soon, however, expect very particular and in-
teresting details.

4. Mr. Davidson, a gentleman of fortune and accomplishments, well known in London, by his interesting descriptions of Jerusalem and Thebes, at the Royal Institution, of which we gave reports, (*Records*, i. 322, ii. 72,) has started for Africa, with the intention of penetrating to Timbuctoo, examining the range of Atlas in his way, and proceeding by Tafalet. He has taken with him an extraordinary man, who was born at Timbuctoo, and whose father was governor of Gana—having been carried as a slave to Jamaica, where he was found at the age of 50, or more, by Dr. Madden. This gentleman was struck with him, in consequence of his astonishing acquirements, as he speaks several African languages with great accuracy, and writes Arabic beautifully. Dr. Madden wrote an account of him to this country, and Mr. Davidson requested, that he should be sent over at his expense. While in London, he was introduced to the Duke of Sussex, who promised him, that if he should prove faithful to Mr. Davidson, he should, on his return to England, be provided for in one of the Royal palaces for life. He left this country, deeply impressed with the kindness he had received, and the last letters from Mr. Davidson, speak of him in the most flattering strains.

5. The last accounts from the Euphrates expedition, which was then at Birr, living in, what with little regard to classical taste, has been called Fort William, were by no means favourable, the greater part of the members of the expedition being in a bad state of health. The enthusiasm, however, of the commander continued unabated.

IV.—*On Chemical Symbols.*

To the Editor of the Records of General Science.

Sir,—It is with regret that I see so little attention paid to the invitation which you gave to the chemical world to discuss the subject of symbols in the pages of your Journal. If you consider the following observations of any importance, perhaps you will give them a place in the *Records*.

Though the importance of symbols appears on all hands to be admitted, yet very few, and these very imperfect, attempts appear to have been made to supply a set, which will be applicable to all branches of chemistry, and which shall be free from objections or defects.

The idea of symbols appears to have originated with Dalton, and to have been employed by him in the elucidation of his immortal discovery, the Atomic Theory. But the variously marked circles, which he employed, though sufficient to illustrate the nature of atomic compounds, are, nevertheless, so evidently inadequate to answer the end for which symbolic representation is required, that no one, I believe, has ever thought of employing them for that purpose. The next step in the improvement of symbols appears to have been by Thomson in his system, as has indeed been observed by yourself, by substituting the initial letters of the substance, for the circular marks of Dalton, and expressing the compound by connecting together the symbols of the substances of which it is composed. This suggestion may be considered as the foundation

of chemical symbols, for it is upon it that all the plans which have hitherto been proposed are based. It may, therefore, be considered as a fundamental rule in *symbolization*, that every simple subtance shall be expressed by the initial letter of its name, or if that be not sufficient to distinguish it, by the two first or by the first followed by the next after it, which will be characteristic of that one substance and of no other.

That this plan of symbolization should be adhered to, is, I believe, the opinion of all chemists.* The use of symbols, however, is not to mark the simple substances alone, but to exhibit to the eye in such a manner as to be quite intelligible, and at the same time in a small space, the composition of chemical compounds. These symbols or marks for the simple substances must, therefore, be combined together, so as to express the compounds, and it is in the method of combination that chemists are at variance.

The first point which is disputed appears to be, whether every substance should constantly retain its symbol, or, whether it be not advantageous to abbreviate the symbols of some of the most common substances, such as oxygen, sulphur, &c. The latter method of which, Berzelius appears to be the inventor, is countenanced by Thomson, Turner, Graham and seems to be almost invariably adopted.

Upon considering the subject, however, this plan appears to be liable to many and serious objections. It had been supposed that oxygen was the only principle capable of forming acids and bases, and, therefore, absolutely necessary to the existence of a salt entering into the composition of both its constituents. It was, therefore, very natural to abbreviate the symbol for a substance which enters into such numerous combinations, as it could apparently be done without injuring the uniformity of the system in the least. But when it was discovered, principally by the ·experiments and investigations of several chemists, and not a little by those of Berzelius himself, that oxygen is not the only substance capable of forming acids and bases, and, therefore, salts; but that chlorine, bromine, iodine, sulphur, selenium, tellurium, and, perhaps, other substances, are capable of forming each a distinct class of salts; this reason for abbreviating the symbol for oxygen falls evidently to the ground, unless we supply an abbreviation also for each of the analogous substances. This, indeed, Berzelius has attempted to do with respect to sulphur, by employing a comma for its symbol; thus evidently admitting the inconsistency of abbreviating the symbol of one of these bodies without doing the same to the rest. But, if oxygen is to be represented symbolically by a *period,* and sulphur by a *comma,* (without venturing to express hypo-sulphurous acid by a *semicolon,*) why should not, on the very same ground, tellurium have a *point of admiration,* selenium ⁻ a *cross,* chlorine an *asterisk,* and bromine and iodine other appropriate characters to represent them symbolically.

* With the exception of Mr. Richard Phillips who includes all such concise methods under one class and order.
                " A Babylonish dialect
                   Of patched and piebald languages."—EDIT.

Is it not much more philosophical as well as more convenient to preserve always the same symbol for the same substance, instead of returning to the old plan of circles, or the still more ridiculous form of the marks of punctuation?

Proceeding, then, on the broad ground, that letters are to form the basis of symbolical representation, and that these letters are to be constantly adhered to, in order to preserve regularity; if we proceed to take a binary compound (to take the most simple form) and consider it in order to express it symbolically, we find, that in every binary compound, one of the elements is electro-negative, and the other electro-positive; and that it is the electro-negative element which marks the substances with which this binary compound may be capable of being combined. This peculiarity, therefore, seems to indicate that the electro-negative element ought to form a prominent part of the symbol. Would it not, therefore, be convenient to place the symbol of the electro-negative element in smaller letters than, and as an index to, the electro-positive one? Thus, to express pro-toxide of iron, I would propose to write Fe^o, and to express chloride of potassium K^{cl}, and to express sulphuric acid S^{3o}, making the figure which is the co-efficient, or indicates the number of atoms of the element, a little larger than the symbol for the element, accord-ing to the plan already proposed by Mr. Hiley, (*Records*, ii. 478.) In this manner, the composition, action and mode of combination of the substance would constantly be kept in view, which is un-doubtedly the great object of symbolic representation. The same plan would also suit acids and bases with a compound electro-positive element; thus, for instance, acetic acid would have for its symbol $(4C\ 2\ H)^{3o}$, and tartaric acid, $(4\ C\ 2\ H)^{5o}$.

Salts, or compounds of an acid and a base, are of course expressed by uniting their formulæ. As long as the salt is neutral, the com-position may be expressed symbolically without difficulty. Thus, the symbol for anhydrous sulphate of soda would be $Na^o\ S^{3o}$, and for carbonate of lime $Ca^o\ C^{2o}$; when, however, we are required to express a salt with excess of acid or base, the case is different. A figure must be placed in connexion with the acid or base as the case may be, which will express the number of atoms of it which enter into the compound. Now, this cannot be placed before the acid or base, for this place may be occupied, as in the case of the yellow iodide of mercury, or of claomel, the symbol for which according to Thomson's view of their composition (which undoubtedly agrees with the atom of mercury, as deduced from the specific gravity of its vapour, and from its specific heat), would be $2\ Hg^l$ and $2\ Hg^{cl}$ respectively. Nor can we place this figure as an index, according to the common method, for the electro-negative element occupies that position more advantageously. It is, however, equally convenient to place it above the symbol of the acid or base. In this manner the symbol for bi-silicate of lime would be $Ca^o\ \overset{2}{Si}^o$, and that for borax $Na^o\ B^{2o}$ $+\ 8\ H^o$.

In still more intricate compounds, as when several salts are com-bined together, as is not unfrequently the case in artificial compounds, and very often in the mineral kingdom, it may be proper to intro-

duce the sign + between the symbol of each salt. Thus, the mineral species garnet which is not unfrequently a compound of

1 atom silicate of alumina
1 atom silicate of lime.*

would have for its symbol $Al^° Si^° + Ca^° Si^°$, and the soda tartrate of potash would be expressed by

$$K^° (4 C \overset{2}{2} H)^{5 0} + Na^° (4 C \overset{2}{2} H)^{5 0}.$$
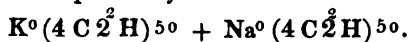
This method of symbolical representation will, I trust, be found to want some of the defects under which the previous ones have laboured, and it seems peculiarly adapted to organic chemistry, the dawn of which seems to be close at hand.

I shall here conclude by apologizing for occuping so much of your time and space. S.

V.—*New mode of heating Apartments.*

AT the Royal Institution on the 11th Instant, Dr. Arnot shewed, that the expense of heating rooms in the usual way by an open fire is enormous, in consequence of the waste of heat. In a chaldron of coals, one half of the heat produced is sent up the chimney, while the remainder radiates into the room; but one half of this is also subsequently sent up the chimney, which is principally occasioned by the width of that aperture. Hence, $\frac{3}{4}$ths of the fuel is actually wasted. In cold countries an open fire cannot be employed because it is not capable of heating a room when the temperature of the air is very low. Stoves are, therefore, used; but an objection to stoves is that they become red hot and send too much heat up the chimney. On this account they are often surrounded by porcelain and brickwork. In manufactories, steam pipes and hot air are employed to impart a regular temperature to rooms, without which the cotton yarn would be injured. Dr. Arnot tried to heat his library by means of a hot water box communicating with the kitchen; but he found the expense (.£30) too great. He then thought of heating the water in the box itself, and thus making it portable; and, lastly, he contrived a hot air box of simple construction which answers the purpose completely. It consists of a square box of plate iron formed of two chambers, which communicate at the top, capable of containing any quantity of air in proportion to the size of the room.
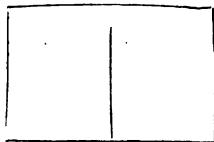
On one side at the bottom there is a tube which conducts fresh air into a small porcelain furnace enclosed in the box. This air after supplying oxygen for combustion passes to the upper part of the box and circulates into the posterior chamber where there is a chimney. To prevent the air from becoming too hot, a valve is adjusted to the

* Thomson's Mineralogy, i. 260.

box formed of a double bar of iron and brass, which opens in proportion to the temperature. Dr. Arnot finds that it requires no attention, that a sufficient quantity of fuel deposited in the furnace in the morning lasts for 24 hours, and thus produces a very great saving of fuel. The heating surface may be increased by having two or more of these boxes at any distance connected by tubes. Dr. Arnot described a method of ventilating rooms by having two parallel tubes, one of them supplied with a fanner to extract the air, and the other with a simular contrivance to force in air.

VI.—*Magnetic characters of the Metals.*

THE opinion of Dr. Faraday, as stated at the Royal Institution in reference to the metals, is, that they are all magnetic, just as they are all capable of being solidified, but that a proper temperature is the desideratum, as with mercury, for the solidification of which a low temperature is required. The analogy is principally derived from the case of iron which loses its magnetic power at an orange heat, and when cooled down regains its attractive power. Nickel exhibits similar properties. When heated and cooled, it retains its negative state long after it has ceased to be visible in the dark. Even when emersed in hot almond oil it loses its magnetic power. This point appears to be between 630° and 640°. Cobalt and chromium are stated in chemical works to be magnetic. Dr. Faraday found that specimens of these metals, which were said to be magnetic, derived that property from the presence of iron or nickel. The result of his experience in respect to chromium is similar to that of Dr. Thomson, who long ago determined that it was not magnetic. Dr. Faraday endeavoured to excite the magnetic power in a number of metals by sinking their temperatures to 60° and 70°, but could not succeed; nevertheless, he is convinced that the only desideratum, in reference to the developement of magnetism in all metals, is the particular magnetic temperature.

VII.—*Halley's Comet.*

THIS remarkable visitor was first seen in the beautiful sky of Italy, on the 5th of August last, at the Observatory of Rome, by Dumouchel and Vico. Its position then, was near ζ of the Bull. On the 21st of the same month, it was observed at Paris, Breslaw, and Naples; on the 22nd at Vienna and Berlin; 23rd at London; 24th at Nimes; 26th at Dublin; 27th at Florence and Bologna; 31st at Yale College, Newhaven, in North America, by Professor Olmsted and Tutor Loomis, its right ascension, being by observation, 5 h. 50·5 m., and its declination N. 24° 46'·8; on the 1st September at Turin and Geneva. By a letter, dated Madras, 27th September, which I have received, it appears, that " no trace of the mysterious body can be found."[*] It was seen by the naked eye at Paris on the 23rd Sept. and at Geneva on the 24th. On the 15th October, with the naked eye, the tail of the comet embraced an extent of 20°, but on the 16th, it appeared to extend only 10° or 12°. On the 30th, it was very

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[*] It was visible in the Bombay presidency on the 6th October.—EDIT.

distinctly visible to the naked eye all over Europe and America. This was 47 days before it reachd its perihelion, which happened on the 16th of November. The previous calculation of Damoiseau gave the 4th of November for this event, that of Pontecoulant the 7th of the same month. But a more complete calculation of the action of the earth, and, especially, the substitution for the mass of Jupiter of the fraction $\frac{1}{1054}$, instead of $\frac{1}{1070}$, rendered it necessary to add 6 days to the previous determination, which brought the number to the 13th, within 3 days of the actual date. When Pontecoulant thus deduced the 13th as the date of the perihelion, he proceeded on the calculation, that 1054 globes similar to Jupiter would be necessary to form a weight equal to that of the sun. The recent observations of Airy have shewn, that it should be 1049, which raises the date of the perihelion from the 13th to the 16th; the difference between calculation and observation being only half a day for 76 years. This remarkable coincidence has raised some doubt. The perturbations produced by the planets upon which the French astronomers made their calculations, were as follow : augmentation of revolution by the action of Jupiter 135,34; diminution by Saturn 51,53; by Uranus 6,07 ; by the earth 11,70 = 66,04 total augmentation. Rosenberg, a German astronomer, considers that the action of Venus, Mercury, and Mars, may produce an acceleration of $6\frac{1}{3}$ days, viz. $5\frac{1}{3}$ days by the action of Venus, and one day by the combined attractions of Mars and Mercury. Pontecoulant asserts, that the action of Venus compensates itself, and that Mars and Mercury cannot produce any such powerful effect as that stated by Rosenberg.

It is natural to inquire, have any new phenomena been observed, or has any additional information been acquired by the visit of the comet of last year ?

1. At the Observatory at Paris, on the 15th October, at 7 o'clock in the evening, by means of a lunar telescope, a sector comprised between two right lines directed towards the centre of the nucleus, was observed a little to the south of the point, diametrically opposite to the tail. The light of this sector greatly surpassed that of all the rest of the nebula. On the 16th, this sector had disappeared, but to the north of the point, diametrically opposite to the axis of the tail, a new sector was observed. On the 17th it remained, but was less bright. On the 21st, at $\frac{3}{4}$-past 6, P. M., three luminous sectors were distinctly seen in the nebula ; the feeblest was situated at the prolongation of the tail. On the 23rd, the sectors had disappeared. Schwabe, of Dessau, calls these sectors secondary tails. Mr. Cooper, observed one such sector in Ireland, on the 19th October ; and Amici noticed the same at Florence on the 13th.

2. It cannot be said that the last appearance of the comet has added any thing to our knowledge of the nature of space. Supposing it to have passed through a resisting medium, it should have arrived at his perihelion sooner than if it moved through a vacuum. Now, on the contrary, according to Rosenberg, it should have been six days later over the results of calculation, apart from all idea of an ether. The difference though much smaller, found by Pontecoulant, is in the same direction.

3. No comets have presented hitherto any phases, so that we were ignorant of the nature of the light of these bodies. It was expected that the intensity would have been determined during the last appearance of Halley's comet, but the remarkable changes which it underwent prevented this from being effected. M. Arago, therefore, adopted another method. On the 23rd October, having applied an apparatus adapted for observation, he saw two images, which presented complementary tints, one red, the other green. By making a half revolution of the telescope upon itself, the red image became green, and vice versa. " Thus the light of the star, was not completely, at least, composed of rays endued with the properties of direct light, peculiar or assimilated ; it contained some light reflected specularly or polarized, that is to say, definitely, some light proceeding from the sun."

VIII.—*Diamonds of the Uralian Mountains.*

PROFESSOR ENGELHARDT, of Dorpat, from an examination of the geological nature of these mountains some time ago, gave it as his opinion, that they contained diamonds ; but Count Polié was the first to discover them, during the travels of Humboldt in that country. M. Parrot has examined 30 of these precious products of the mine, which were formerly confined to India and Brazil. All of them have 24 triangular faces, more or less irregular and mostly striated. They belong to a kind of rhomboidal dodecahedron, of which each rhomboid is here as it were folded up on the diagonal which passes through the obtuse angles. Several of them have a yellow tinge. They are all oblong and more or less flattened. Two of them, one weighing $\frac{1}{4}$ carat, and the other $\frac{2\frac{1}{2}}{3}$ carat, contained small black bodies. They are not crystallized, but may be compared to bodies in the form of moss as in agates. They are not acted on by the magnet. Parrot conceives, that as " chemical analysis has proved that the diamond is composed almost entirely of carbon and a very little hydrogen like vegetable carbon, it is very probable, that the black matter of these diamonds is a species of vegetable carbon, under an uncrystallized form. It is well known also, that jewellers who receive diamonds for cutting, deprive them of the black streaks which are frequently attached to them externally by ignition, which would not happen if these were not formed by a compound of carbon and hydrogen. This observation on the exterior streaks which form a mass with the exterior layers of the diamond, furnish us with a new analogy, for admitting with a high degree of probability, that *the black matter of the two diamonds which we have examined, is a substance, consisting of carbon and hydrogen, and that these diamonds themselves are still imperfect diamonds.* But what appears to decide the question is, that the small black masses in the crystal which we examined are isolated and entirely enclosed in the crystalline mass, without touching any of its faces or angles. If they were heterogeneous bodies, formed previous to crystallization, they would have been placed upon some base and covered there with the crystalline mass, as occurs, for example, in quartz, agates, &c., where the foreign

body, whether crystallized or not, appears to proceed from some angle or face of the crystal. Thus, the black streaks are homogeneous with the mass before it was modified to form the crystal; they are a remnant of the carbon and hydrogen which had not obtained the transparency when the remainder already transparent was crystallized."

In eight of the diamonds which he examined, Parrot observed fissures or cracks in various directions, which he conceives, can only be explained, by supposing them to have been first exposed to heat and then suddenly cooled, and, therefore, that they have been subjected to volcanic action.—*Memoires de L'Academie Imperiale de St. Petersbourg*, iii. 21, 6th Series, 1835.

IX.—*Statistics of the Canadas.*

ACCORDING to Dr. Kelly, the diseases and deaths in Lower Canada from 1820 to 1827, were as follow : fevers 2669, deaths 35 ; pneumonia 979, deaths 30 ; rheumatism 550 ; phthisis and hemoptysis 130, deaths 74 ; catarrh, acute and chronic 1233, deaths 10 ; dysentery and diarrhæa 1195, deaths 2 ; other diseases 9113, deaths 66. Total diseases 15,869, deaths 217. The mean annual mortality from 1820 to 1831, in Lower Canada, was 1·333 per cent. In Upper Canada 1·253 per cent. In both Canadas from 1810 to 1822, the total number of men was 99·483. Sick 114·883. Total mortality 2461 = 2·54 per cent.

It appears from a census made of the country, that the number of births to a marriage, in Lower Canada, is 6. The lowest rate of mortality was in 1799 and 1816, being 1 in 52·72, and 1 in 54·3 respectively. The greatest mortality was in 1810 and 1820, or 1 in 33·14, and 1 in 34·5 respectively. The mean annual temperature at Quebec in 1832 was 35°·87. The highest range of the thermometer 85°. The lowest 25°. The winds blew 220 days from the west, 121 from east, and 25 variously. The number of snowy days was 63, rainy 96, dry days 210. The mean temperature for 1832, 33 and 34 was 35°·87. The mean annual heat of wells situated 180 and 200 feet above the tide waters of the St. Lawrence was 42°·74.—*United Service Journal, Oct.*, 1835.

X.—*Mode of preserving minute Animals.*

EHRENBERG, of Berlin, has been enabled, by rapid exsiccation upon small plates of mica to form a collection of nearly 300 infusorii, belonging principally to those which he has published. These objects are arranged upon small plates, similar to those which are employed for examining the scales of butterflies. He has preserved the form and colour, not only of the armed Radiator, but also of the softest Radiator and Polygastrica, even those of the genus Monades. He has also preserved the tissues of plants, the spermatozoes, the different kinds of blood globules with their nuclei, and the lymph, chyle and nervous tubes of a great many animals.—*Journ. de Chim. Medic.* i. 492.

Meteorological Journal,

At the Manse of the Parish of Abbey St. Bathan's, Berwickshire, Lat. 55° 52' N. Long. 2° 23' W. at the height of about 450 feet above the sea. By the Rev. JOHN WALLACE.

FEBRUARY.

DATE 1836.	THERMOMETER IX A.M.	X. A.M.	III. P.M.	X. P.M.	HYGROMETER (Leslie's) IX AM	X AM	III PM	X PM	BAROMETER at 32° Fahr. IX A.M.	X. A.M.	III. P.M.	X. P.M.	Rain in Inches Weekly	Direction of Wind at X. A.M.	REMARKS.
Mo. 1 (Full M.)	36	36¼	36	33	9	9	12	6	28·202	28·219	28·249	28·286		W. N. W.	Very strong wind with unsettled sky, evening nearly calm.
Tu. 2	30½	31¼	33	27¼	4	5	6	9	28·223	28·266	28·290	28·544		S. W.	Calm, cirri on a blue sky, in the evening wind veering northward.
We. 3	36¼	36	36¼	35¾	10	11	8	0	28·956	28·976	29·097	29·356		N. N. W.	Strong wind, cloudy, and clouds in rapid motion, in evening some rain.
Th. 4	38¼	40	39¼	32¼	5	5	11	0	29·700	29·723	29·682	29·825	0·109	E. N.	Calm, sky overspreading with large masses of cloud in motion, evg. deposition.
Fr. 5	31	34	47¼	38	5	11	16	0	29·746	29·738	29·531	29·302		W. S.	Brisk wind, hoar frost in morning with clear sky, gradually clouding, evg. rain.
Sa. 6	38¼	40	39½	35½	20	20	21	9	29·210	29·208	29·240	29·203		W. N. W.	Brisk wind, hazy, in the evening the stars shining dimly.
Su. 7	33½	34	34	28¼	13	13	4	3	29·250	29·275	29·338	29·408		S. by E.	Calm, morning hoar frost, A.M. masses of clouds, P.M. cloudy, evg. deposition.
Mo. 8	36	37¼	41¼	42¾	13	9	3	7	29·225	29·187	28·980	28·948		W. N. W.	Gentle wind, cirrostra. overspreading sky, light clouds floating, evg. windy.
Tu. 9	36	39¼	40½	35	13	9	15	10	29·028	29·052	29·096	29·221		W. N. W.	Brisk gusty wind with driving showers, evening clear, deposition.
We. 10	34¼	35	36¼	32	7	10	12	1	29·166	29·160	29·050	28·912		W. N. W.	Gentle wind, cirrostratus overspreading sky, evg. wind boisterous with snow.
Th. 11	29¼	30¼	31¼	33¼	13	12	11	8	29·450	29·472	29·583	29·453	0·250	W. N. W.	Gentle wind, sky nearly cloudless, evening overcast, strong boisterous wind.
Fr. 12	38¼	40¼	37	43	17	20	23	8	29·280	29·333	29·522	29·751		W. N. W.	Very brisk wind, patches of cirrostratus, evening calm and nearly cloudless.
Sa. 13	34	34½	34½	43	11	11	7	8	29·222	29·222	29·676	29·663		W. by S.	Brisk wind, cloudy, A.M. tendency to rain, evg. wind gusty with dull sky.
Su. 14	41½	42	44	43¼	15	15	19	16	29·703	29·720	29·690	29·715		W. by S.	Wind strong but occasionally hushed into calms, cloudy, frequently overcast.
Mo. 15	42¼	43¼	44½	42¼	7	5	22	8	29·744	29·760	29·644	29·610		S. S. W.	Gentle breeze, soft and fine.
Tu. 16 (New M.)	44¼	44½	44½	31¼	6	5	14	14	29·441	29·441	29·352	29·329		W.	Brisk wind with occasional showers, evening wind strong and veering N.
We. 17	31¼	33¼	38¼	37	14	16	24	19	29·417	29·458	29·564	29·723		N. by W.	Boisterous wind with disturbed sky, A.M. snow-showers, evening clearing.
Th. 18	35¼	36¼	35¾	32¼	16	15	15	7	29·908	29·931	29·957	29·979	0·011	N. by W.	Brisk wind, cloudy, A.M. slight snow-showers, evening calm with dull sky.
Fr. 19	33½	35	36¼	33¼	6	11	18	7	29·900	29·886	29·796	29·787		W. S. W.	Gentle wind, cirri and cirrostratus prevalent, evening calm with snow-showers.
Sa. 20	33¼	37	40¼	41¼	9	13	9	9	29·907	29·907	29·880	29·818		W. N. W.	Calm, fine, cirri and cirrostratus prevalent, evening clear.
Su. 21	40	40¼	43	43	10	12	10	5	29·572	29·548	29·446	29·355		SW. by W	Very gentle breeze, cloudy, P.M. lowering, in the evening sudden gusts.
Mo. 22	39½	41¼	42	33	11	15	19	5	29·220	29·216	29·153	29·149		SW. by W	Very gentle breeze, fine, some floating cumuli, evening hazy. deposition.
Tu. 23	33½	33¼	39½	30¼	6	17	21	2	29·072	29·061	28·936	28·993		SW. by W	Copious hoar frost morning, calm, hazy, cumulous and cirrocumulous clouds.
We. 24	33½	33¼	33½	27¼	6	6	1	0	28·835	28·826	28·718	28·582		W.	Calm with snow all day, wind veering northward.
Th. 25	34½	35¼	36	27¼	1	3	9	2	28·450	28·473	28·487	28·587	1·578	E. W.	Strong wind, hail-showers until 9 A.M., calm, cirrostratus, evg. clear, frosty.
Fr. 26	17¼	20¼	32	32	9	6	4	3	28·652	28·690	28·702	28·698		N. W.	Calm with intense frost, occasionally misty, evening overcast.
Sa. 27	33¼	36¼	35¼	33½	6	6	3	0	28·723	28·727	28·733	28·865		N. E.	A.M. gentle breeze, P.M. wind strong and gusty, heavy clouds, snow-showers.
Su. 28	34	34	35¼	23¼	2	2	0	3	28·945	28·963	28·983	29·046		N. E.	Wind strong and gusty, sky overcast, frequent showers of sleet and snow.
Mo. 29	35	36	38	34	9	5	10	4	29·045	29·045	29·025	28·925		S. E.	A.M. calm, fine, P.M. cloudy, evening overcast, wind heard in gusts.
Means.	36·2	36·4	38·5	34·5	9	10	13	6	29·214	29·221	29·221	29·243	1·948		

Means { Therm. 35·4 / Hygr. 8· / Bar. 29·252 in. } for the Month.

Mean temperature of spring water 45°·5
Mean point of deposition 29°·7
Moisture in a cubic inch of air = 4°·25 grs.

RECORDS

OF

GENERAL ·SCIENCE.

Article I.

Memoir of Dr. Thomas Young. By M. Arago.

(*Continued from* page 251.)

The finest discovery of Dr. Young, and one which will im-
mortalize his name, was suggested by a very trifling object—
soap-bubbles—so brightly coloured, and so light, that they
had scarce escaped from the pipe of the scholar, when they
became the sport of the most imperceptible currents of air.

I have, then, been enabled to trace up to the play of a
child the discovery which I am about to analyze, with the
conviction that it would not suffer from such an origin. In
every case, it is not necessary to recal the recollection
either of the apple, which, on being detached from the
branch, and falling accidentally at the feet of Newton, sug-
gested the ideas in this great man of the simple laws which
regulate the motions of the heavenly bodies; or, of the
frog and bistouri to which natural philosophy owes the
remarkable pile of Volta. Without mentioning, in fact,
the name of soap-bubbles, I would suppose that a natural
philosopher has chosen for the subject of his experiments
distilled water, that is to say, a liquid whose transparency
has become proverbial, and which, in its state of purity,
exhibits some shades of blue and green scarcely sensible,
unless through a great thickness. I should ask then, what
would one think of the veracity of a person, who, without
any other explanation, announced, that to this limpid water

he could, at pleasure, communicate the most resplendent colours? That he could render it violet, blue and green, that he could make it yellow like the skin of the lemon, red like scarlet without affecting its purity, without mixing it with any foreign substance, without changing the proportions of its principal gaseous constituents. Would not the public consider our natural philosopher as unworthy of all credit, if, after these remarkable results, he added, that in order to produce colour in water, it is sufficient to give it the form of a pellicle, that *thinness* is in truth synonymous with colour; that the transition from each shade to the most different tint is the necessary consequence of a simple variation in the thickness of the liquid layer; that this variation in the transition from red to green, for example, is not the thousandth part of the thickness of a hair. Yet these incredible results are the inevitable consequences of the chance colours presented by liquid bubbles blown out, and even by thin plates of all kinds of bodies.

To understand why such phenomena have daily been presented to philosophers for 20 centuries without exciting attention, we have only to consider to how few persons nature has imparted the valuable faculty of being astonished at the proper time.

Boyle first penetrated into this fruitful mine. He confined himself, however, to the minute description of the varied circumstances which give birth to the iris. Hooke went farther. He thought that these kinds of colours were derived from the intersection of the rays, or to use his own words, " in the intersection of waves reflected by the two surfaces of the thin plate." This was a proof of genius, it must be admitted; but it could not be properly taken advantage of at a time when the complex nature of white light was still unknown.

Newton made the colours of thin plates the object of his favourite study. He devoted to it a whole book of his celebrated treatise on optics; he established the laws of their formation by an admirable chain of experiments, which no one has since surpassed. In explaining with homogeneous light the regular evidence of which Hooke made mention, and which is produced round the point of contact of two lenticular glasses placed above each other, he proved that

for each species of simple colour there exists in thin plates of every kind, a series of thicknesses increasing where no light is reflected. This was a capital result. It contained the key to all the phenomena.

Newton was less happy in the theoretical views which this remarkable observation suggested to him. To talk with him of a reflected ray of light being in a fit of easy reflexion, or of a ray which passes completely through a plate being in a fit of easy transmission, is nothing else than to announce in obscure terms, what the experiment with the two lenses had taught us. The theory of Thomas Young escapes this criticism. Here, no fit of any kind is admitted as a primary property of rays. The thin plate is found always in a similar condition to a thick mirror of the same substance. If, in certain points, no light was seen, Young did not conclude that reflexion had ceased there; he supposed that in certain directions from these points, the rays reflected by the second face in proceeding to meet the rays reflected by the first are *completely annihilated*. It is to this conflict of rays that the author gave the famous term of *interference*.

Certainly this is a remarkable hypothesis. It occasions great surprize to find the night in the blaze of sunshine, in points where the rays of this body are freely sent; but who would have imagined that darkness could be produced by adding light to light.

A natural philosopher is justly in a glorious position, when he can announce some result in direct opposition to the commonly received opinions; but it is necessary without delay to bring forward demonstrative proofs, otherwise we shall be likened to oriental writers, whose reveries amused the sultan *Schahriar* for a thousand and one nights. Young was not so prudent. He shewed, first, how his theory was adapted to the phenomena, but without proceeding beyond probabilities. When, subsequently, he obtained true proofs, the public had objections which he could not overcome. However, the experiment upon which Young made his discovery rest, would not admit of the shadow of a doubt.

Two rays coming from the same source, and proceeding by slightly unequal routes crossed each other at a certain

point in space. In this point a leaf of fine paper was placed.
Each ray taken separately made it shine with much effect,
but when the two rays were re-united, when they arrived
simultaneously on the leaf, then light disappeared,—the
most complete night succeeded the day.

Two rays are not always annihilated at their point of in-
tersection. Sometimes a partial weakening only is observed.
Every thing depends on the length of their course, and
this by very simple laws, the discovery of which, at any
period, would have been sufficient to immortalize a phi-
losopher.

The differences in the direction which produce among
rays a confluence accompanied by their entire destruction,
have not the same value for light of different colours.
When two white rays cross, it is then possible that one of
their principal constituents, the red for example, exists
alone in the condition of destruction; but white less. The
red constitutes green. Thus the interference of light is
manifested by the phenomena of colour, and thus, the
different elementary colours are exhibited without the
assistance of a prism to separate them. It cannot fail to
be remarked, that there does not exist a single point in
space where a thousand rays, proceeding from different
origins, do not cross each other after more or less oblique
reflexions, and one can perceive at a glance, the whole
extent of the unexplored region which these interferences
open to the investigation of philosophers.

When Young published this theory, many phenomena of
periodical colours were already offered to observers; it
should be added, that they had resisted all explanation.
In the number we may reckon the rings formed by reflexion,
not from thin pellicles, but from plates of thick glass
slightly curved; the iridescent bands of different sizes, by
which the shadows of bodies are bounded exteriorly, and
sometimes covered interiorly, which Grimaldi observed
first, which subsequently attracted the genius of Newton
without success, and the complete theory of which was
reserved for Fresnel; the red and green arcs which are
observed in a greater or less number, immediately, below
the seven prismatic colours of the chief rainbow, and which
appear so completely inexplicable as to have been passed

over in physical treatises; and lastly, those crowns with constantly changing diameters which often appear to surround the sun and moon.

When I recal to mind the limited number of persons who can appreciate scientific theories unless they immediately apply them, I could not terminate this enumeration of phenomena which characterize the more or less numerous series of periodical colours, without mentioning the rings so remarkable for the regularity of their form, and by the purity of their lustre with which all bright light appears surrounded, when it is examined through a mass of molecules or filaments of equal dimensions. These rings, in fact, suggested to Young the idea of an extremely simple instrument which he called *eriometer* with which the measurement of the smallest bodies can be readily effected. The eriometer still so little known to observers has this immense advantage over the microscope, that it gives at once the mean size of millions of particles comprized within the field of vision. It possesses besides the singular property of becoming negative when the particles differ too much from each other, or, in other words, when the question of determining their dimensions cannot be answered.

Young applied his eriometer to measure the globules of the blood of different classes of animals, those of the dust furnished by different species of vegetables, to the measurement of the fur employed in the manufacture of stuffs, from that of castor the finest of all, to that of the fleeces of the common flocks of the county of Sussex, which, placed at the extremity of the scale, are composed of filaments four and a half times larger than the hair of the castor.

Before Young, the numerous phenomena of colour which have just been pointed out were not only unexplained, but there was nothing written on the subject. Newton, who had long been occupied with the subject, had not, for example, perceived any connexion between the iridescence of their plates and the bands of diffraction. Young found these two species of coloured striæ to be only the effects of interference. Subsequently, when chromatic polarization was discovered, he brought up in some measurements of thickness some remarkable numerical analogies, well fitted

to lead to the presumption, that, sooner or later, this curious kind of polarization would be attached to his doctrine. There is still, however, a great hiatus to fill up. Our ignorance of important properties of light prevents us from understanding the phenomena in certain crystals where double refraction produces peculiarity by the destruction of light, resulting from the intersection of bundles of it. But it is to Young that the honour of having opened this field of inquiry belongs; it is he who first decyphered these optical hieroglyphics.

The word hieroglyphic, viewed no longer metaphorically but in its natural acceptation, carries us to a field which has been the scene of many animated debates. I have had some hesitation in exposing myself to the feelings which this question has raised. The secretary of an academy exclusively occupied with the exact sciences might, without any apology, leave the consideration of this philological question to more competent judges. Besides, I hesitated, I confess it, on finding myself disagreeing on several important points with the illustrious philosopher.

All these scruples vanished when I reflected, that the interpretation of the hieroglyphics is one of the most beautiful discoveries of the age; that Young has mixed up my name with these discussions; that, to examine lastly, if France had any pretensions to this new glorious title, was to increase my present object, it was to act like a proper citizen. I am well aware of what may be objected to in these sentiments. I am not ignorant that cosmopolitism has its good side; but, in truth, by what name could I not stigmatize it, if, when all surrounding nations enumerate with satisfaction the discoveries of their children, I should be prevented from finding out in the land of my birth, among my countrymen, without wounding their modesty, proofs that France has not degenerated; but that it still contributes its proportionate addition to the vast depôt of human knowledge. I proceed then to the question of the Egyptian writing, I proceed to it free from all pre-conceived notions, with the firm desire of acting justly, and the anxious wish of reconciling the rival pretensions of two philosophers whose premature deaths have filled the whole of Europe with a just subject of regret.

Men have conceived two completely distinct methods of writing. The one employed by the Chinese is the hieroglyphic system, the second, in actual use among all other nations, bears the name of the alphabetic or Phonetic system.

The Chinese have no (properly so called) letters. The characters which they employ in writing are true hieroglyphics; they represent not sounds nor articulations, but ideas. Thus *house* is expressed by means of a special character which did not change even when all the Chinese denominated a house, in the language spoken, by a word totally different from that now employed. If this statement should excite surprise, it is only necessary to refer to our cyphers which are also hieroglyphics. The idea of unity added seven times to itself is expressed everywhere in France, England, Spain, &c. by means of two round figures placed one above the other vertically and touching at a single point. But on seeing this ideographic sign the French call it *huit*, the English *eight*, the Spanish *ocho*. Every one knows that it is the same with compound numbers. Hence, it may be observed, if the Chinese ideographic signs were generally adopted, as the Arabic figures are, each would read in his own language the works presented to him, without requiring to know a single word of the language spoken by the authors who wrote them. Thus, there would be no alphabets.

> " *Celui de qui* nous vient cet art ingénieux
>   De peindre la parole et de parler aux yeux,"

having made the capital remark, that all the words of the richest spoken language are composed of a very limited number of sounds or elementary articulations, invented signs or letters, to the number of 24 or 30 in order to represent them. With the assistance of these signs combined in different ways, every word which strikes the ear may be written even without being aware of the signification.

The Chinese or hieroglyphic writing appears to have been the infancy of the art. It does not require, however, as was said in former times, in order to learn to read, even in China, the long life of a studious mandarin.

Remusat, whose name I cannot mention without regretting his death, as one of the greatest losses which literature has for a long time experienced, has proved both by his own experience and that of his pupils, that the Chinese may be learned like every other language. It is a mistake to suppose that hieroglyphic characters can only be employed to express common ideas. Some pages of the *Yukiao-li* or the *Two Cousins* are sufficient to shew, that the most subtile matters do not escape in Chinese writings. The principal defect of this method of writing is, that it affords no means for expressing new names. A letter written from Canton to Pekin could have told that on the 14th of June, 1800, a very memorable battle saved France from great danger; but it could not tell in purely hieroglyphic characters, that the plain where this glorious event took place was near the village of Marengo, and that the victorious general was Buonaparte. A people, among whom the communication of proper names from one city to another could only take place by means of messengers, cannot have exceeded the rudiments of civilization. Such is not the case, however, with the Chinese. Hieroglyphics constitute the majority of their writings; but sometimes, and especially when it is wished to write a proper name, they are deprived of their ideographic meaning in order to express sounds and articulations, and to form true letters.

The question of priority, which the Egyptian methods of writing has originated, can be easily explained and comprehended. We find, indeed, in the hieroglyphics of the ancient people of the Pharaohs, all the methods which the Chinese at present make use of.

Several passages in Herodotus, Diodorus Siculus, and Saint Clement of Alexandria shew that the Egyptians employed two or three kinds of writing, and that in one them, at least, the symbolic characters or representations of ideas bore a conspicuous part. Horapollon has preserved the signification of a certain number of these characters. Thus, we know that the *sparrow-hawk* represents the *soul;* the *ibis* the *heart;* the *pigeon* (which is remarkable) a *violent man;* a *flute* a *madman;* the number *sixteen, pleasure;* a *frog* an *imprudent man;* the *ant, knowledge;* a *slip-knot, love; &c.*

The signs thus preserved by Horapollon form only a very small portion of the 800 or 900 characters which are found on monumental inscriptions. The moderns, Kircher among others, have attempted to increase their number. Their efforts have not led to any useful result, unless to shew into what errors the best informed men may be led when they abandon themselves without restraint to their imagination. From the absence of date, the interpretation of the Egyptian inscriptions appeared, for a long time, to all thinking men, an absolutely insoluble problem; when, in 1799, M. Boussard, a talented officer, found in the trenches which he had formed about Rosetta, a large stone covered with three series of perfectly distinct characters. One of these series was Greek. This, notwithstanding some mutilations, afforded the information, that those who had ordered the monument to be raised had enjoined that the *same inscription* should be delineated in three characters, viz., in sacred characters or Egyptian hieroglyphics, in local or common characters and in Greek. Thus, by a fortunate accident, philologists were presented with a Greek text with its *translation* into the Egyptian language, or, at least, a transcription with the two sorts of characters employed on the banks of the Nile.

This Rosetta tablet, which has since become so celebrated, and which M. Boussard presented to the Institute of Cairo, was carried off when the French army evacuated Egypt. It may now be seen in the British Museum at London, " where it figures," says Thomas Young, " as a monument of British valour." Laying aside the idea of valour, the celebrated philosopher might have added, without exhibiting too much partiality, that this bi-lingual monument bore witness, in some small degree, of the enlightened views which had presided over all the details of the memorable Expedition to Egypt, and also of the indefatigable zeal of the illustrious philosophers, whose labours often executed under the fire of grape-shot, have added so much to the glory of their country. The importance of the Rosetta inscription struck them so forcibly, that, rather than abandon this precious treasure to the dangers of a sea-voyage, they set themselves about producing representations of the original by copper-plates, and lastly, by plaster and sulphur casts. It may be added, that the antiquarians of

every country have become first acquainted with the Rosetta stone from the French designs.

One of the most distinguished members of the Institute, M.Sylvestre de Sacy, in the year 1802, first entered upon the investigation which this bi-lingual inscription opened to philologists. He devoted his attention only to the Egyptian text in common characters. He there discovered the groups which represent different proper names, and their phonetic nature. For, in one of the two modes of writing at least, the Egyptian had signs of sounds and of true letters. This important result was never contradicted, when a Swedish man of science, M. Akerblad, perfecting the work of the French philosopher, had assigned, in all probability, approaching almost to certainty, the individual phonetic value of the different characters employed in the translation of the proper names, which the Greek text made known.

The purely hieroglyphical (or supposed so) part of the inscription remained untouched; no one had attempted to decypher it.

It was here that Thomas Young first declared, as if by a kind of inspiration, that in the number of signs sculptured on the stone, and representing, whether entire animals, or fantastical beings, or instruments and products of art, or geometrical figures; that such of these signs as were enclosed in elliptical brackets correspond to the proper names of the Greek inscription, in particular, to the name of Ptolemy, the only one which remained entire in the hieroglyphical transcript. Young then immediately explained, that in this special case of enclosure or cartouche, the signs did not represent ideas but sounds. Subsequently, he endeavoured to assign, by a minute and very delicate analysis, an individual hieroglyphic to each of the sounds communicated to the ear by the name of Ptolemy on the Rosetta tablet, and in that of Berenice on another monument.

The following, if I mistake not, are the principal points in the researches of Young upon the systems of writing among the Egyptians. No one, it is commonly said, had perceived them, or, at least, pointed them out, before the English philosopher. This opinion, though generally admitted, appears to me contestable. For it is certain, that in the year 1766, M. de Guignes in a printed memoir had

shewn that the Egyptian cartouches contained all the proper names. In the same memoir also, any one may see the arguments which this accomplished oriental scholar employs to establish the opinion which he had embraced with respect to the constant phonetic nature of the Egyptian hieroglyphics. Young has, however, the priority upon one single point, for he made the first trial to decompose the groups of cartouches into letters, in order to give phonetic value to the hieroglyphics composing the name of Ptolemy in the Rosetta tablet.

In this investigation as might be expected, Young furnished new proofs of his great penetration; but misled by a false system his efforts could not be completely successful. Thus, he sometimes attributed a simply alphabetical character, at other times a syllabic, or even a dissyllabic value without accounting for this remarkable mixture in characters of different kinds. The fragment of the alphabet published by Dr. Young contains, therefore, truths and errors; but the faults abound so much that it would be impossible to apply the meaning of the letters to any other inscription than to that of the two proper names from which they were derived. The word *impossible* so rarely occurred in the scientific career of Young that we must hasten to justify ourselves. · I state, therefore, that since the composition of his alphabet, Young believed that he had read on an Egyptian monument the word *Arsinoe*, which his celebrated competitor has since clearly proved to be *Autocrator*, and that he read *Evergete* in a group instead of *Cæsar*.

The work of Chompollion, in reference to the phonetic value of the hieroglyphics, is simple and homogeneous, and does not appear to lead to any uncertainty. Each sign is equivalent to a simple vowel or a simple consonant; its value is not arbitrary; every phonetic hieroglyphic represents a physical object, the name of which, in the Egyptian language, begins with the vowel or consonant which it represents.

The alphabet of Chompollion once modelled from the Rosetta tablet, and from two or three other monuments serves for reading inscriptions entirely different; for example, the name *Cleopatra* on the obelisk of Philoe carried sometime ago to England, and where Dr. Young, provided

with his own alphabet, could perceive nothing. On the temples of Carnac, Compollion read twice *Alexander;* on the zodiac of Denderah an imperial Roman title; on the great edifice above which the zodiac was placed, the names and sirnames of the emperors Augustus, Tiberius, Claudius, Nero, Domitian, &c. Thus, on one side we find, every reason removed for continuing the keen and continued discussion which the age of these monuments had produced, and on the other hand, it is completely proved, that under the Roman sway hieroglyphics continued to be used on the banks of the Nile.

The alphabet which has already afforded so many unexpected results, when applied either to the great obelisks of Carnac, or to the other monuments which are also recognized to belong to the time of the Pharaohs, presents us with the names of several kings of this ancient race, and names of Egyptian divinities; and further, with *substantives*, *adjectives* and *verbs* of the Coptish language. Young was, therefore, deceived when he regarded the phonetic hieroglyphics as a modern invention, and when he advanced the opinion, that they were only employed for the transcription of proper names, and of proper names foreign to Egypt.

M. de Guignes, and especially M. Etienne Quatremere, established on the contrary a real fact of great importance, which the reading of the inscription of the Pharoahs inscriptions has strengthened by the most irresistible proofs, since they prove that the Coptic language was actually that in use among the subjects of Sesostris. The facts have now been stated. I shall now confine myself to a few brief observations by way of strengthening the consequences which appear to me to result necessarily from them.

<div align="center">(<i>To be continued.</i>)</div>

<div align="center">ARTICLE II.</div>

Notices of some Minerals. By THOMAS THOMSON, M. D., F. R. S. L. & E., &c., Regius Professor of Chemistry in the University of Glasgow.

<div align="center">I.—NACRITE AND TALCITE.</div>

SOME specimens, which I have lately received from Ireland by the kindness of Dr. Scouler, Professor of Mineralogy to

the Royal Dublin Society, has enabled me to ascertain that I have confounded together, in my mineralogy, (vol. i. p. 244.) two distinct species of minerals under the name of *nacrite*. For the sake of distinguishing these two species from each other, I restrict the name *nacrite* and apply it only to a mineral from America, which I analysed and described in the year 1827. To the other mineral from Ireland, I shall apply the name *talcite;* as it was probably to it that this name was given by Kirwan.—See his *Mineralogy*, vol. i. p. 149.

Nacrite.—The only specimen of this mineral which I have seen, I received about ten years ago from Dr. Torrey of New York. It was labelled *Green mica in mica slate from Brunswick, Maine.* The specimen consisted of light green coloured scales scattered unequally through a rock composed chiefly of quartz with abundance of iron pyrites interspersed.

The colour of the scales is white with a beautiful shade of light emerald green.

Lustre splendent and pearly.

Composed of plates which are flexible; but not elastic. When viewed under a microscope they have some thickness.

They have one very distinct cleavage parallel to the broad faces of the scales. They shew some obscure indications of two other cleavages, which would indicate an oblique four-sided prism as the primary form of their crystals.

Translucent.—Feel not unctuous.

Very soft.—Specific gravity 2·788.

After ignition they still retain their pearly lustre, but have lost their green colour and become silvery white.

Fuses with difficulty before the blow-pipe. Its constituents are as follow:

Silica, ...	64·440	60·20
Alumina, . . .	28·844	30·83
Protoxide of iron,	4·428	3.55
Water, ...	1·000	5
	98·712*	99·58†

* By my analysis.

† Yellow earthy talc from Merowitz, analysed by John.—Jour. des Mines, xxiii. 384.

The atomic constituents deduced from these analyses are

Silica, ...	31·16	2·34
Alumina, . . .	13·28	1
Protoxide of iron,	0·88	0·066

There is an excess of silica, because the grains of nacrite occcur in quartz, which it is impossible to exclude completely. From this I am disposed to consider nacrite as a bi-silicate of alumina.

Talcite.—The specimens of talcite from Ireland are from the county of Wicklow, where it occurs crystallized in granite. I have received it also in scales interspersed through a soft matter having somewhat the aspect of chlorite. The locality of this last specimen is Strathbane in the county of Tyrone.

The scales are silvery white without any of the green shade which characterizes nacrite.

The crystals are in large four-sided prisms with rough surfaces, the faces of which meet at an angle of about 91°.

Lustre of crystals dull; of scales splendent and pearly.

Scales translucent, crystals opaque.

Hardness 2·25. Specific gravity 2·6918.

When the crystals are pounded they appear to be composed of white silvery scales.

The constituents of talcite are as follow:

				Atoms.
Silica, ...	46·000	44·55	22·27	1·48
Alumina, . . .	35·200	33·80	15·02	1
Protoxide of iron,	2·880	7·70	1·54	
Lime, ...	9·608	1·30	0·37	
Magnesia, . . .	——	3·30	1·32	
Protox. of mangan.,	3·944	2·25	0·45	
Water, ...	2·000	6·65	5·91	⅓
	99·632*	99·55†		

Calculating from the scales, which are obviously purer than the crystals, the mineral is a sesqui-silicate of alumina. The quantity of oxide of iron, lime, magnesia, oxide of manganese and water vary so much in the two specimens analyzed, that they cannot be considered as essential to the constitution of the mineral.

* The crystals analyzed by Dr. Short.
† The scales analyzed by Mr. J. Tennant.

II.—HOLMITE.

Sometime in last October, I received from Mr. Finch a small specimen of a mineral which he had brought from Warwick, in the State of New York near the Hudson river.

In America it was distinguished by the name of *bronzite.* It was subjected to a chemical analysis by one of my practical pupils. But as he met with some unexpected difficulty, I was induced to examine the few grains that remained. I detected in it zirconia; but the quantity in my possession prevented me from being able to subject it to an accurate analysis, having exhausted my specimen in ascertaining the nature of the constituents. Fortunately, about the beginning of the present year, I got a small box of minerals from Dr. Holmes of Montreal, and, among others, there was a pretty large specimen of the supposed *bronzite.* I requested my friend and pupil Mr. Richardson to analyze it, which he did with his accustomed accuracy.

This mineral being obviously new, I have given it the name of *Holmite* as a small tribute to Dr. Holmes of Montreal, to whom I am indebted for so many new and curious minerals from Canada and the United States of America.

Colour dark reddish brown. Lustre semi-metallic.

Texture laminated with only one perceptible cleavage. The plates or leaves into which it splits are elastic like those of mica; but cannot be obtained so thin.

Transparent when in thin plates.

Hardness 6. Specific gravity 3·098.

Before the blowpipe loses its colour and becomes opaque. With carbonate of soda no change. With borax fuses into a colourless transparent bead, containing within it a skeleton of silica. The phenomena with bi-phosphate of soda are the same, except that the bead in the reducing flame acquires a greenish yellow tinge. Its constituents are,

		Atoms.
Silica,	19·35	9·68
Alumina,	44·75	19·88
Zirconia,	2·05	0·54
Peroxide of iron,	4·80	0·96
Protoxide of manganese,	1·35	0·30
Lime,	11·45	3·27
Magnesia,	9·05	3·62
Water,	4·55	4·04
Fluoric acid,	0·90	0·72
	98·25	

If we admit the fluoric acid to have been in combination with lime, that base will be reduced to 2·55 atoms. The atoms of bases amount to 27·85; while those of silica are 9·68. Now the third part of 27·85 is 9·28. Hence, it is evident, that the mineral consists of tri-silicates or of three atoms of base combined with one atom of silica. The formula for its composition will be

$$11 \text{ Al}^3\text{S} + 2 \text{ Mg}^3\text{S} + 1\tfrac{1}{2} \text{ Cal}^3\text{S} + \left\{ \begin{array}{c} \text{Zr} \\ f \\ \text{mn} \end{array} \right\} {}^3\text{S} + 2 \text{ Aq.}$$
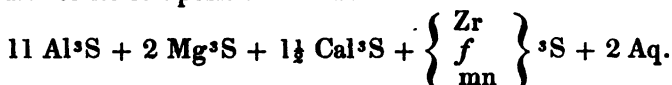
It is a quadruple aluminous salt, and its place may be between *mountain leather* and *pearl-stone* in vol. i. p. 390 of my Mineralogy.

If, by *bronzite* be understood what is usually called *schiller spar*, then holmite is a quite distinct mineral.

III.—ANTHOPHYLLITE AND SCHILLER SPAR.

In a parcel of minerals which Dr. Holmes, of Montreal, was so obliging as send me during the course of the present winter, there was a pretty large specimen, the locality of which was stated to be Perth, Upper Canada. It consisted of a congeries of imperfect crystals having a good deal of the aspect of anthophyllite. Its specific gravity was 2·707, and its constituents were found to be,

		Atoms.
Silica,	57·60	28·8
Alumina,	3·20	1·42
Protoxide of iron,	2·10	0·46
Lime,	3·55	1·01
Magnesia,	29·80	11·9
Water,	3·55	3·1
	99·3	

Hence, the constitution is,

$$4 \text{ Mg S}^2 + (\tfrac{1}{2} \text{ Al} + \tfrac{1}{3} \text{ Cal} + \tfrac{1}{6} f) \text{ S}^2 + \text{ Aq.}$$
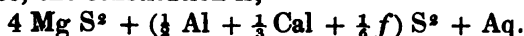
If the reader will turn to page 207, vol. i. of my Mineralogy, and compare the analyses of anthophyllite, he will be satisfied that the American mineral is an anthophyllite as its external characters indicate. But in the specimens of anthophyllite analysed by Vopelius, L. Gmelin and myself, it will be seen, that there were not less than 13 parts

in the hundred of protoxide of iron; while in the American mineral there is only 2·1 per cent. Hence, I conclude, that protoxide of iron is not an essential constituent of anthophyllite. As the lime and alumina are wanting altogether in one of the specimens analyzed, they cannot be essential constituents. The great difference in the amount of the water and its absence in one of the specimens renders it unlikely that it enters into the chemical constitution of anthophyllite. Hence, I am disposed to conclude, that *anthophylle*, if it could be obtained free from all foreign matter would be an anhydrous bi-silicate of magnesia represented by Mg S².

Schiller spar on the contrary is a hydrous bi-silicate of magnesia or Mg S² + Aq.

We need not be surprized after this, that schiller spar is specifically lighter than anthophyllite, and that there exists a certain resemblance between the two minerals.

IV.—DEWEYLITE.

I got a small specimen of this mineral from Dr. Holmes; but unfortunately, he forgot to specify its locality, and merely stated, that it had been analyzed in America, and that its constituents were

$$
\begin{array}{lll}
\text{Silica,} & . & . & . & 40 \\
\text{Magnesia,} & . & . & 40 \\
\text{Water,} & . & . & . & 20 \\
\hline
& & & & 100
\end{array}
$$

The specimen constituted an amorphous mass about double the size of a pigeon's egg.

Its texture is granular, and its colour a light yellowish white. Fracture splintery.

Translucent on the edges. Lustre resinous. Easily reduced to powder.

Hardness 2·5. Specific gravity 2·2474.

Before the blowpipe per se, it becomes white and opaque. With carbonate of soda it fuses into a bead having quite the appearance of enamel, white while hot, but assuming a red tint on cooling. With borax, fuses with slight effervescence into a transparent colourless bead. With biphosphate of soda it does not fuse either in the reducing

or oxydizing flame; but the assay assumes the appearance
of enamel, just as when fused with carbonate of soda.

The constituents of this mineral are,

		Atoms.	
Silica, ...	41·42	20·71	8·8
Magnesia, . . .	23·53	9·41	4
Soda, ...	6·25	1·56	0·66
Alumina, ...	4·47	1·98	0·84 } 1·01
Oxide of cerium, . .	3·57	0·40	0·17 }
Peroxide of iron trace,	—	—	—
Water, ...	19·86	17·05	7·23

$$99·10$$

From these constituents, we may deduce the following
formula for its constitution,

$$4 \text{ Mg S}^{1\frac{1}{2}} + (\tfrac{4}{5} \text{ Al} + \tfrac{1}{5} \text{ Cr}) \text{ S}^{1\frac{1}{2}} + \text{NS}^2 + 7 \text{ Aq.}$$
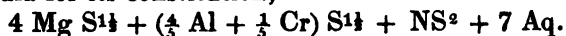
No known mineral species agrees with this formula in its
constitution. Were it not for the presence of NS^2 it might
be considered as differing only from precious serpentine,
by containing twice the quantity of water that enters into
the constitution of that mineral. At all events, *Deweylite*
must constitute a new mineral species.

V.—AMPHODELITE.

This name has been given by Nordenskiöld to a mineral
found by him in the limestone quarry of Lojo, in Finland;
and Berzelius gives a short description of it, together with
its analysis by Nordenskiöld, in his Jaresbericht for 1833,
p. 174. Last year I received a small specimen of a mineral
from Dr. Holmes, of Montreal, which struck me as resem-
bling the amphodelite of Nordenskiöld, so far as I could
judge from the description of Berzelius. The locality was
Bytown, Upper Canada.

The specimen was an amorphous mass not much exceed-
ing the size of a pigeon's egg. I could detect in it no cleav-
age, nor any appearance of crystallization; while in Nor-
denskiöld's specimen two cleavages could be observed meet-
ing at an angle of 94° 19′.

The colour of this mineral was partly white and partly
light rose red, distributed unequally, and running gradually
into each other, so as to leave an uncertainty where the one

begins and the other terminates. . Small olive-green stains, in dots, pervade the mass, most probably derived from some foreign matter; but too minute in quantity, and too intimately mixed with the stone, to be separated.

Texture granular. Lustre between glassy and resinous. Translucent on the edges.

Hardness about 6, or not much softer than felspar. Specific gravity 2·8617.

It was analyzed in my laboratory with great care by Mr. John Tennant. The following table shews the constituents of the American variety, and of the Lapland amphodelite as analyzed by Nordenskiöld.

	Nordenskiöld.	Tennant.
Silica, . . .	45·80	45·80
Alumina, . .	35·45	26·15
Lime, . . .	10·15	16·25
Magnesia, . .	5·05	2·95
Protoxide of iron,	1·70	4·70
Water, . . .	1·88	2·00
	100	97·85

The silica in both specimens is precisely the same. The sum total of the bases is the same in both, though the weight of the individual constituents differs. The following formulas represent the constitution of both varieties:—

$$\text{Lapland } 3 \text{ AlS} + (\tfrac{11}{21} \text{ Cal} + \tfrac{8}{21} \text{ Mg} + \tfrac{2}{21} f)\, \text{S}$$

$$\text{American } 2 \text{ AlS} + \begin{Bmatrix} \text{Cal} \\ \text{Mg} \\ f \end{Bmatrix} \text{S}^2$$

I think it probable [that both of these varieties contain foreign matter, and that the differences between them are owing to this circumstance.

VI.—WEISSITE.

The name *Weissite* was given by Trollé Wachtmeister to a mineral which occurs in kidney-shaped pieces, about the size of a hazel-nut, in chlorite slate, at Erick Matt's mine Fahlun, in Sweden. For a description of this mineral I refer the reader to my *Mineralogy*, vol. i. p. 282.

Towards the end of last year, I got from Dr. Holmes, of Montreal, a specimen of a mineral from Potton, in Lower

Canada, which he distinguished by the name of *Grey chlorite.* This specimen bears a near relation to the Weissite of Trollé Wachtmeister.

Its colour is ash grey, with a slight tint of blue.

The structure is slaty—Lustre resinous. Fracture even. Opaque. Does not feel soapy; yet in Canada it goes by the name of soapstone.

Hardness 1·75. It is readily scratched by the nail, and appears softer than selenite.

Specific gravity 2·8263.

It was analyzed, at my request, by Mr. John Tennant, in my laboratory, who obtained the following constituents:—

		Atoms.
Silica, ...	55·05	27.5
Alumina, . . .	22·60	10
Protoxide of iron,	12·60	2·8
Lime	1.40	0·4
Magnesia with trace of manganese,	5·70	2·2
Water, ...	2·25	2
	99·6	

The formula indicated by these proportions is $2 \, AlS^2 + (\frac{2}{3} f + \frac{1}{3} Mg) \, S^2$

Trollé Wachtmeister found the constituents of his Weissite to be,

		Atoms.
Silica, ...	59·69	29·84
Alumina	21·70	9·64
Magnesia	8·99	3·60
Protoxide of iron, .	1·43	0·31
Protox. of manganese,	0·63	0·14
Potash	4·10	0·68
Soda	0·68	0·17
Oxide of zinc . . .	0·30	0.05
Water with ammonia,	3·20	2·84
	100·72	

The formula from these proportions is,

$2 \, AlS^2 + (\frac{8}{11} Mg + \frac{2}{11} K + \frac{1}{11} f) \, S^2 + \frac{1}{3} Aq.$
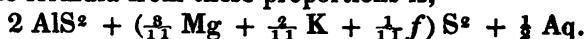
I think it very probable, from comparing the analyses of these two minerals, that the only essential constituents

are AlS², all the others being accidental impurities. If that supposition be admitted, Weissite will be merely an impure variety of the *Nacrite* described at the beginning of these notices.

VII.—MAGNESITE.

In page 178, vol. i., of my *Mineralogy*, I have given an imperfect description of this mineral, which occurs in the marl limestone round Paris, and which was first noticed by M. Alex. Brogniart, and analyzed by Berthier. As I had never seen a specimen of the mineral, my description was necessarily imperfect. But, in a box of minerals, which I received last October from Dr. Holmes, there was a small specimen, labelled *anhydrous deweylite?* This specimen agreed exactly with magnesite, as far as I could judge from Berthier's description. I may add (as Berthier has forgotten to mention it) that the specific gravity is 2·0964, and that the mineral is opaque. It was analyzed by Mr. J. Tennant. I shall place Berthier's analyses of magnesite beside it, that the reader may judge how far they agree.

	Berthier	Tennant.
Silica,	54	50·70
Magnesia,	24	23·65
Alumina,	1·4	3·55
Protoxide of iron,	—	1·70
Water,	20·1	20·60
	99·5	100·2

The French specimen is the purest; but the formula for both, leaving out what a comparison of the two analyses shows to be impurities, is Mg S³ + 2 Aq.

The mineral is, of course, a hydrous tersilicate of magnesia.

ARTICLE III.

On the Causes of the Motion of the Blood in the Capillary Vessels. By Dr. POISEUILLE.*

WHEN the globules of the blood in the capillaries of the mammiferæ are examined, they are found to possess diffe-

* Bibliotheque Universelle, Nov. 1835..

rent velocities, even in the same vessel. Some of them have two simultaneous motions—one of rotation, the other of translation ; while others remain motionless for a time. Two globules, presenting at first the same velocity, only preserve by accident the distance which separates them, and, if the motion be such as to permit us to follow the same globule, we can observe it sometimes in the same capillary vessel presenting these different phases of motion. The velocity of the globules in the capillaries is less than in the arteries and veins; it is seldom greater. This remark extends also to a capillary vessel which rises immediately from an artery, or which proceeds directly into a venous trunk. These different phenomena lead to the conclusion that the globules are endued with a spontaneous motion, or, rather, that the cause of the flow of blood through the capillaries is different from the cause which regulates the motion of the blood in the large vessels.

Dr. Poiseuille has endeavoured to examine with great attention the causes of the motion of the blood in those parts of vessels which have been isolated from the action of the heart by means of ligatures, or separated from the body by cutting instruments, and then to study the influence of the heart and arteries upon the capillary circulation.

He has established, by a great number of experiments, that the calibre, which the arteries and veins present, proceeds from the pressure of the liquid which they contain; that their coats are constantly distended by the blood which they receive; that these vessels tend to collapse suddenly, in consequence of the elasticity of their coats, as soon as the cause of their dilatation is removed.

The large arteries and veins, as well as the small ones, possess this property; but, besides, the diameter of the last gradually diminishes when they cease to receive blood. This retraction is sometimes so great, that the mysenteric vessels of the frog, salamander, young rats and mice, are reduced to two-thirds of their original diameter. He has also ascertained that, *cæteris paribus*, this retraction is more decided in the arteries than in the veins. These facts being known, it is easy to determine the motions of the blood in parts which have been separated from the trunk either by ligature

or by a cutting instrument—motions which even yet are designated by the title of circulation.

In fact, an attentive examination of this pretended circulation shews us, that the part being in a horizontal plane, the motion of the globules in the capillaries is totally at an end; that all the vessels, arteries and veins, carry the blood from the extremities to the amputated surface; that this motion, becoming more and more slow, ceases after the expiration of some time, and at the same time the organ presents a much smaller quantity of blood. These motions result, then, simply from the approximation of the coats of the vessels towards their axis; they necessarily, therefore, drive the blood towards their open extremities. The tail of the frog, the foot of the same animal, the mysentery of very young rats, of young mice, separated from the trunk by a cutting instrument, have presented constantly the same phenomena. This pressure which he has established with regard to the blood of animals, exists also in the liquids of vegetables; he also believes that this kind of circulation, which may be observed in the stipula of the *Ficus elastica*, detached from the trunk, is due to the same cause.

The action of gravity, as well as that of heat, are also causes, but confined within more narrow limits of the motion of the globules in parts separated from the trunk, especially when the blood has not yet coagulated in the vessels.

Numerous experiments made, 1st, upon the heads of the salamander and frog, animals in which the circulation is, as it were, suspended at pleasure, shew that it is established gradually from the centre to the circumference; 2nd, upon the foot of the frog, dividing the crural vessels; 3rd, upon the mesentery of the frog and salamander, by cutting the heart; 4th, upon the mesentery of young rats and mice. All these experiments, of which several are confirmed by those of the two celebrated physiologists, Haller and Spallanzani, have convinced Dr. Poiseuille that the heart and elasticity of the arterial coats are the *sole agents* in the capillary circulation in question.

In resting upon the preceding facts, that is to say, the action of the heart and arteries, and the tendency which the latter have to collapse when they are not sufficiently

dilated by the tide of blood projected from the heart, the constant *jerking, intermittent,* and *oscillatory* circulation, which precede the death of an animal, are easily explained; the cause of the *retrogade circulation* presented by the arteries after the death of the animal and that of the heart is similar.

Having cleared up these points, the author passes on to the examination of the causes of the irregular motions of the globules which he has observed in the capillary vessels.

If we study the course of the blood in the arteries and veins of the frog, of very young rats and of young mice, we observe, in proceeding from the axis of the vessel to the coats, that the velocity of the globules is totally different. In the centre, their velocity is at a maximum; it diminishes gradually as we approach the coats. In the immediate neighbourhood of the coats, a very transparent space can be observed, which is generally occupied by serum; this space is equal to about the $\frac{1}{8}$ or $\frac{1}{16}$ of the diameter of the vessel. This transparent part of the vessels observed by Haller and Spallanzani as being occupied by serum, has been again noted by Blainville.

Since some of the globules, rubbing against each other, are projected into this transparent part of the vessels, the globules placed in the middle possess a very slow motion, and they cease to move when they are almost in contact with the coats of the vessel. The globules which are nearest to this transparent part have a double motion of rotation and translation; they roll, if the expression may be used, over this part of the serum.

From these observations, the author concludes that the interior of the vessels is lined with a layer of serum at rest. Since this layer is immoveable in its immediate contact with the coats of the vessels, every time that a globule is placed there it will be at rest, or, rather, its velocity will be more or less diminished, according to the portion of the globule immersed in it. Now, in the capillaries the globules move between two layers of serum. Hence, their motion ought to be less rapid than in the large vessels, since they require to overcome the inertia of this layer.

If a globule is partly in the layer, this portion of the globule will be at rest, while its remainder, placed in the

axis of the vessel, will acquire a certain velocity; then the globule will move round its own axis, in order to acquire its normal velocity in following the centre of the vessel. If of two globules, one is placed in advance of the other in the layer, the former will pursue its course, and the latter will be delayed, and the motions described will be presented.

The labours of M. Girard upon the flow of liquids in tubes of small diameter have established, in most tubes susceptible of being softened by the liquid moving in them, the existence of a similar layer. The author passed through tubes of very small diameter, liquids holding in suspension opaque bodies; and, having examined this current by a microscope, he found this layer immoveable, and of a thickness much smaller than that obtained by the calculations of Girard.

Hence, the author concludes, that the blood transported by the vessels of the heart to all parts of the body does not impinge against the coats; that a layer of serum, by its state of rest, guards the coats from any such effects. Besides, we can conceive the importance of this immoveable layer of serum lining the coats of the vessels in the act of nutrition, since the recent experiments of Müller of Berlin have demonstrated, that the fibrin is held in solution by the serum.

Dr. Poiseuille has farther studied the influence of cold and heat upon this layer of serum. The following experiment shews the result. At the temperature of 77°, he examined the circulation in the foot of a frog, and, in the vessel where the foot was placed, he deposited pieces of ice. In the large vessels, the transparent part of the serum obviously increased in thickness; the globules in immediate contact with it moved more slowly; the three orders of vessels, arteries, capillaries and veins, preserved their diameters sensibly; the velocity in the capillaries was considerably diminished, and in some of these vessels it became insensible; during six or eight minutes, for example, the circulation in the capillaries of the other foot of the frog preserved its normal velocity: and it was not till a quarter of an hour after the submersion of the first foot in the iced water that the velocity of the blood in the second foot, placed in the atmosphere, was diminished, in consequence

of the temperature of the whole mass of blood being sunk.
The ice in the vessel was replaced by water at the tempera-
ture 100°, and the velocity of the globules became then so
great, that their form could scarcely be distinguished.   In
young rats, the cold, applied only for a few minutes,
stopped the circulation in the capillaries of the mesentery.
It gradually resumed its powers, and acquired its normal
rhythm after the ice was withdrawn.

Thus the diminished velocity of the capillary circulation
by cold, and its greater rapidity by the action of cold, are
naturally interpreted by the increase in the thickness of this
layer in the first place, and its diminution in the second.

These results completely correspond with those of M.
Girard on the variation in the thickness of the layer which
lines the 'coats of inert tubes, when the temperature
increases or diminishes.

We know that certain animals, such as fishes, and some
amphibious mammalia, are sometimes immersed nearly
262½ feet (80 metres) beneath the surface of the water, and
then support a pressure of from seven to eight atmospheres.
It is important, therefore, to know how this layer acts, and
at the same time to observe the modifications of the capil-
lary circulation under such pressure.   With this object in
view, the author has constructed an apparatus, to which he
has given the name of *Porte-objet pneumatique*.   A short
description will afford an idea of it, and develope the
results which may be derived from its use.   It consists of a
strong box of copper; the top and bottom are formed of
crystal, fitted into grooves placed in the sides.   One of the
extremities of this box carries a copper tube, which contains
sometimes a barometer tube, and sometimes a manometer
for compressed air; the other extremity presents a large
opening by which the animals are introduced.   To this
extremity sometimes a suction pump is adapted, and some-
times a forcing pump.   The animal, prepared in such a
way as to allow the capillary circulation to be seen, is
placed in the instrument, and the apparatus placed under
the microscope.   We can then observe the modifications
which may introduce in the capillary circulation a more or
less considerable pressure.   In salamanders, frogs, tad-
poles, young rats and mice, the arterial, venous, and capil-

lary circulation have not presented any remarkable change, even when raising the pressure rapidly to 2, 3, 4, 6 and 8 atmospheres, and reciprocally. Farther, the circulation has continued to preserve the same rhythm even under a pressure of some centimètres (39 inches) in salamanders, frogs and tadpoles. On placing in the apparatus very young rats and mice (it is well known that the mammalia, during the first days of their birth, may remain some hours without breathing) the circulation can be seen perfectly, *in vacuo*. How absurd, then, is the opinion of these philosophers who consider that, without atmospheric pressure, circulation cannot go on; but atmospheric pressure, combined with the motions of respiration, are accessory causes of the flow of the blood, as Dr. Poiseuille has shewn in another memoir.

From these experiments, he infers, that the thickness of the layer of serum, the existence of which is due to the affinity subsisting between the coats of the vessels and the serum, a thickness which varies so remarkably from heat and cold, is independent of surrounding pressure, that the contractions of the heart preserve their normal rhythm, whatever the pressure is.

Several tubes of *chara*, placed in this apparatus, had presented, under a pressure, varying from 209 (7·8 inches) to 600 centimètres (23·6 inches) the same modes of circulation; and the motions of some infusorii contained in the water of the chara, such as *vorticelli, potiferæ*, &c., were executed with the same facility as under the influence of the atmosphere.

ARTICLE IV.

On the Connexion between Refracted and Diffracted Light.
By PAUL COOPER, Esq.

(From a Paper read before the Royal Society, 8th May, 1834.)

IT is remarkable, that nearly two centuries have elapsed, during which, other sciences have sprung up and arrived at maturity, without producing any considerable alteration in the material theory of light, as it was left by its illustri-

ous founder: no essential part of it has been invalidated by subsequent experiment; nor has it received much accession of strength by the filling up of the intervals, which, it must be admitted, is required to give it support from connexion. It still remains, like an unfinished magnificent building, with valuable materials for its completion scattered around it. Refraction, reflexion, inflexion, polarization and other branches into which the science of optics has been divided, though so evidently united in the operations of nature, are still unconnected in theory, and remain distinct objects of investigation.

This want of connexion in the material theory of light has laid it open to various attacks; and such has been the recent success of a rival theory, that some of its most zealous supporters appear to be wavering.

The following quotations from Sir David Brewster's life of Sir Isaac Newton will shew the importance, as it relates to this question, of establishing a connexion between the theories of refraction and diffraction, or inflexion; and it is with this object in view that I now claim your indulgence.

" By this mode of observation, he (Fresnel) made the remarkable discovery, that the inflexion of light *depended on the distance of the inflecting body from the aperture, or from the focus of divergence,* the fringes being observed to dilate as the body approached that focus, and to contract as it receded from it, their relative distances from each other, and from the margin of the shadow continuing invariable," p. 104.

" The various phenomena of inflexion, which had so long resisted every effort to generalize them, having thus received so beautiful and satisfactory an explanation from the undulatory doctrine, they must, of course, be regarded as affording to that doctrine the most powerful support, while the Newtonian hypothesis of the materiality of light is proportionally thrown into the shade. It is impossible, indeed, even for rational partiality to consider the views of Newton as furnishing any explanation of the facts discovered by Fresnel," p. 105.

If we look through a prism at an unclouded sky, it presents a white surface, bounded by red and yellow fringes on one side, and blue and violet fringes on the other. These

fringes are complimentary to each other; but, at first view, if we suppose them to be connected, we are as much at a loss to account for their great distance from each other, which includes the whole field of view, as the early experimentalists in galvanism were to account for the appearance of oxygen and hydrogen, in the proportions which form water, in distant parts of the apparatus. A system of mutual intermediate compensations, satisfactorily accounts for both these appearances: the colours which are deficient in the fringes on one side are not transferred by the prism to the fringes on the opposite side, but to the white light immediately adjoining, which surrenders equal portions of the same colours to form another adjoining surface of white light, until, by a succession of such transfers, the complimentary colours appear at the opposite side, where there is no white light from which these colours can be compensated.

When we look at the sky, or any other white object, with the naked eye, we see three images of it, which are superposed so as to form one white image; and this image appears in the proper position of the object; but when we look at it through a prism, we see the three images in three different positions, neither of which is its true position; the deviation being less in the red image than in the green, and less in the green than in the violet image.

If, then, under this new arrangement, we interpose an object, either before or behind the prism, we do not intercept the same part of the different images, as we do when these images are formed of parallel rays, but equal portions of different parts of them; so that, if the deviation of the images from each other be equal to the breadth of the intercepting object, we still see the whole of the sky, or whatever other object we are looking at, in the different colours: for the part of the object, not seen in one colour, is seen in the others; and it is these separated parts of the three images which form the fringes observed in light refracted by the prism.

It clearly appears from these and other experiments, that parallel rays are not essential to the formation of white light; and that, if the colours are properly proportioned, the intersection of the rays may be made at any angle, without impairing its purity.

The best method of preparing white refracted light, for the purpose of experiment, is to receive the light of the sun, refracted by a prism of considerable breadth, upon a screen placed at a short distance from the prism. It will be found that all objects placed in this light will form shadows on the screen fringed with complimentary colours, which will increase in breadth by increasing the distance of the object from the screen. The edges of the interposed object have nothing to do with the production of these coloured fringes; they arise simply from the different direction of the rays of different colours, and the consequent difference in the position of their shadows.

We may increase the surface of white refracted light to any extent by a proportional increase of the breadth of the prism; and, if the divergent light could be made to occupy the whole of the circle of which the prism is the centre, no distance whatever would produce the least appearace of of colour; for, although the differently coloured rays would diverge at different angles, they would intersect each other at every point in the proportion required to form white light. Colours make their appearance in this light merely from the want of continuity; and it will be found, in every case, that by bringing the two fringes towards each other, a distance equal to the breadth of the interposed object, by means of which the continuity is interrupted, the white surface will resume its original purity by the superposition of complimentary colours.

Having given my views of the character of refracted light in my papers on the number and character of the colours that enter into the composition of white light, it will be unnecessary for me to enlarge upon the subject here; there is, however, one part of its character that I had no occasion to notice in the former inquiry, which is of importance in the present; and I shall make a few observations upon it before I proceed to my principal object.

If we form surfaces of white refracted light with prisms of the same materials, but with different refracting angles, upon screens placed at equal distances from the prism, we shall find that the breadth of the fringes by which these surfaces are bounded will vary with the angle of the prism; and that the fringes upon the shadows of objects, at the

same distance from the screen, will be broader in proportion as the refracting angle is greater.

There is no certain standard, therefore, for refracted light; and, although its distinguishing qualities are observable only when it is in a state of considerable divergence, all light which has lost the parallelism of its rays by refraction must come under this denomination.

Refracted light, then, includes the light of the sun as it is transmitted by our atmosphere; and, indeed, most of the light with which we are acquainted. The light of the sun is frequently so much refracted, particularly in the morning and evening, as to form distinct fringes, which may be often observed upon the bars of windows, or upon the edges of other objects placed to intercept it.

I shall now proceed to shew the connexion between this light and the diffracted light of Grimaldi and Newton.

Diffracted light is formed either by admitting common light through a small hole, or through a lens of very short focus. The experiments made with light formed in these different ways, render it probable that the effect produced by the two methods of operating upon it, is nearly the same.

A lens differs from a prism, not only in its circular form, which is calculated to refract the light of different colours into concentric circles, but also in its having different refracting powers at different distances from the centre; in consequence of which, the light that passes through it does not open in the centre and form distinct concentric rings of colours, but diffuses itself over the whole surface, forming a circular image of white light, bounded by fringes of the most refrangible colours.

The light which has passed through a lens, then, is refracted light; but it is unequally refracted: it is so refracted, that the whole of the light transmitted by the lens converges to the same point, and, consequently, it must be composed of a central ray, which, as it proceeds in a right line, undergoes no refraction, surrounded by circles of rays gradually becoming more highly refracted as they recede from the centre.

Light thus constituted will endeavour to form a distinct circular spectrum with every circle of rays; the whole of

them, however, will so interfere with each other, in conse-
quence of the very gradual difference of refraction, that,
while their continuity is uninterrupted, they will form a
surface of white light by superposition.

. In order to observe the different phenomena of diffracted
light, let a lens L L, of very short focus, be fixed in the
window shutter, M N, of a dark room; and let R L L be a
beam of the sun's light transmitted through the lens.
This light will be collected in the focus F, from which it
will diverge in lines F C, F D, &c., and form a circular
image of diffracted light on the screen, C D.
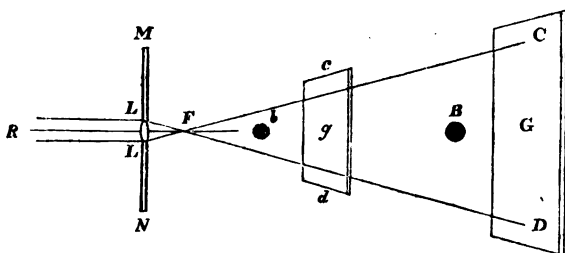
" The shadows of all bodies whatever held in this light,
will be found to be surrounded with three fringes of the
following colours, reckoning from the shadow :

First fringe.—Violet, indigo, pale-blue, green, yellow,
red.

Second fringe.—Blue, yellow, red.

Third fringe.—Pale-blue, pale-yellow, pale-red."

" Let a body B be now placed at the distance B F from
the focus, and let its shadow be received on the screen
C D, at a fixed distance from the body B, and the follow-
ing phenomena will be observed :—

1. Whatever be the nature of the body B with regard to
its density or refractive power, whether it is platina or the
pith of a rush, whether it is tabasheer or chromate of lead,
the fringes surrounding its shadows will be the same in
magnitude and in colour, and the colours will be those .
given above.

If the light R L is homogeneous light of the different
colours in the spectrum, the fringe will be of the same
colour as the light R L, and they will be broadest in red

light, smallest in violet, and of intermediate sizes in the intermediate colours.

3. The body B continuing fixed, let us either bring the screen C D nearer to B, or bring the lens with which we view the fringes nearer to B, so as to see them at different distances behind B. It will be found that they grow less and less as they approach the edge of B, from which they take their rise. But if we measure the distances of any one fringe from the shadow at different distances behind B, we shall find that the line joining the same point of the fringe is not a straight line, but a hyperbola, whose vertex is at the edge of the body; so that the same fringe is not formed by the same light at all distances from the body, but resembles a caustic curve formed by the intersection of different rays.

4. Hitherto we have supposed that B has been held at the same distance from F; but let it now be brought to C, much nearer F, and let the screen C D be brought to c d, so that b g is equal to B G. In this new position, where nothing has been changed but the distance from F, the fringes will be found greatly increased in breadth, their relative distances from each other and from the margin of the shadow remaining the same."

These observations, descriptive of the character of diffracted light, are extracted from Sir David Brewster's Optics, *Cabinet Cyclopædia,* chapter the 11th.

With regard to the first observation : It has already been shewn, that the fringes formed on the edges of the shadows of bodies in refracted light, proceed from the different direction of the rays of different colours, arising from their difference of refrangibility; and that these fringes are totally independent of the refractive power of the body which forms the shadow.

In explaining the second of these observations, it will be necessary to keep in view, that the rays of light of different colours are quite independent of each other; and, consequently, that, when only one colour is transmitted, it will take precisely the position it would occupy if other rays of different colours were present.

It has been before observed, with reference to refracted light, that the breadth of the fringes at equal distances

from the object which forms the shadow, are proportioned to the degree of refraction; in order, then, to account for the greater breadth of the less refrangible colours, we have only to shew, that, whatever may be the position of the object B, with regard to its distance from the point F, the less refrangible colours that pass its edges, must necessarily proceed from a part of the lens where they are more highly refracted, than the more refrangible colours which pass with them.

In whatever position we fix the object B, on the line R, formed by the central ray, a little attention will discover to us, that the white light immediately adjoining its edges, which, on its arrival at the screen, forms the fringes, is composed of rays of the different colours, which proceed from different parts of the lens; for every compound ray transmitted by the lens, is refracted in the order of the refrangibility of the different colours of which it is formed; and its component parts must be separated to distances from each other, in proportion to their degree of refraction, and the distance from the centre of divergence; but, as there is a succession of rays, supplied by the breadth of light which falls on the surface of the lens, gradually differing from each other in their degrees of refraction, the more refrangible colours which are separated by greater divergence are replaced by similar colours proceeding from a part of the lens where they are less refracted: the white light, therefore, which surrounds the central ray, is every where formed by the intersection of different colours, the least refrangible of which are the most highly refracted. The fringes, consequently, under circumstances in other respects similar, if observed apart from each other, as in the case which is the object of inquiry, will be broadest in red light, smallest in violet, and of intermediate size in the intermediate colour.

That white diffracted light is formed by the intersection of coloured rays, the least refrangible of which pass through a part of the lens of higher refracting power than the more refrangible rays which cross them, may be shewn from the following consideration:—The surface of white light upon the screen C D, is formed by the superposition of enlarged images of the lens L L, in red, green, and violet-

light. Now, if the different colours were equally refrangible, the three images would correspond in size, and every part of the surface would be white; but, in consequence of the different refrangibility of the different colours, the violet image is larger than the green, and the green larger than the red; the violet, therefore, extends beyond the green, and exhibits a ring of this colour unmixed with any other; concentric with it, there is a ring of the green extending beyond the red, where the white light commences; but this ring is superposed by the violet, which converts it to blue: It is evident that these rings, or fringes, are formed of the most refrangible rays which pass nearest the edge of the lens, where the light is the most highly refracted; and, consequently, that the rays of these colours which intersect the most highly refracted part of the less refrangible colours, to form the white light, adjoining the fringes, must pass through the lens at a distance from its edges proportioned to the breadth of the fringes. The other parts of the white surface are formed upon the same principle; but the difference of refraction, arising from this cause, decreases as we approach the central ray, where it ceases; the images at this point being in exact correspondence with each other, and unrefracted.

(*To be continued.*)

ARTICLE V.

On some Hydrargyro-Cyanurets. By Mr. GEORGE HENRY JACKSON.

To the Editor of the Records of General Science.

SIR,—I shall feel obliged by the insertion in your excellent Journal of the following account of the *Hydrargyro-Cyanurets.*

The fact that the cyanurets are capable of uniting with one another just as two oxidized bodies, and of forming definite compounds, has been long known; but those *double cyanurets* which have been hitherto examined are but few, and, amongst these few, perhaps the compounds of the two *cyanurets of iron*, with the *electro-positive cya-*

2 A 2

nurets are the only ones that have been thoroughly examined; but there cannot be a single doubt as to the existence of numerous other *double cyanurets*.

The following salts were *all* prepared by mixing the *simple cyanurets* of which they are composed in atomic proportion, taking care at the same time to exclude atmospheric air as much as possible; the mixed solutions were then evaporated *in vacuo* with sulphuric acid. I have styled the following salts the *Hydrargyro-cyanurets*, because they are composed of *bi-cyanuret of mercury* and the *electro-positive cyanurets*.

I. *Hydrargyro-cyanuret of potassium* is a compound that crystallizes in octohedrons, white and perfectly transparent, from the mixed solution of *bi-cyanuret of mercury* and *cyanuret of potassium*; in order to ascertain the composition of this salt, the following mode of analysis was used : As most of the *double cyanurets* retain a portion of their water of crystallization with such violence, it can only be completely expelled by employing a temperature sufficient for the decomposition of the salt; therefore, the following mode of ascertaining the *water of crystallization* appeared to me the best :

10 grs. of this salt (in crystals) were heated in a tube connected by means of a cork with another tube filled with chloride of calcium in small fragments; the latter weighed, before the experiment, 38·50 grs.; after sufficient heat had been applied to completely decompose the salt, taking care at the same time that the *mercury* did not sublime, the tube containing the chloride of calcium was removed from the tube in which the *decomposed salt* was, and weighed, when it was found that the increase of weight was 0·02 grs.; too small a quantity to be considered as water of crystallization, being most likely water mechanically enclosed within the crystals, for they *decrepitate* strongly on being heated.

Very dilute sulphuric acid was then added to the *decomposed salt* in the tube, and the mixture warmed; the solution of *sulphate of potassa* thus obtained was filtered, and the filter well washed with distilled water; the solution and washings were then evaporated to dryness in a weighed platinum crucible; the *sulphate of potassa* was then *ignited*, and a small quantity of *carbonate of ammonia* was

supported on a piece of platinum foil at the mouth of the crucible, so as to prevent the possibility of any *potassa* existing in the state of *bi-sulphate ;* the crucible, after the ignition, was again weighed, when, on subtracting its former weight from the latter, it was found that the weight of the *sulphate of potassa* was 1·23 grs. = 0·93 grs. of *cyanuret of potassium* = 1 equivalent.

In order to estimate the quantities of *mercury* and *cya-nogen*, the following process was adopted :—

10 grs. of the salts in crystals were dissolved in *water*, to which a solution of *sulphuret of potassium* was added very slowly, until no further precipitate took place; by this means *the whole* of the *mercury* was precipitated as *bi-sul-phurets*, which was placed on a weighed filter, and well washed with *distilled water ;* after thoroughly drying the filter it was weighed, when, on deducting its former weight from the latter, the difference was 8·0 grs. : *Bi-sulphuret of mercury* = 9·10 grs. *bi-cyanuret of mercury* = 2 equivalents. The solution filtered from the *bi-sulphuret of mercury* contained *the whole* of the *cyanogen* present in the salt, now in the state of *cyanuret of potassium ;* to this solution *nitrate of silver* was added until *the whole* of the *cyanogen* was precipitated as *cyanuret of silver*, which was placed on a weighed filter, and well washed with distilled water; it was then transferred to a weighed platinum capsule, and converted into *chloride of silver*, by acting on it with *muriatic acid*; after driving off the excess of muriatic acid by heat, the filter was burnt in the capsule, and the whole heated to a temperature short of fusion; on deducting the weight of the capsule, and the ashes of the filter, from the weight of the whole, the weight of the *chloride of silver* was 10·27 grs. = 1·89 grs. of *cyanogen*, = 5 equivalents, exactly corresponding to the calculated quantity in the former analysis.

Thus, then, 10 grs. of this salt contain,

> Of *Cyanuret of potassium*, 0·93 = 1 *equivalent.*
> ,, *Bi-cyanuret of mercury*, 9·10 = 2 *equivalents.*
> *Water*, 0·02

$$\overline{10·05}$$

The *equivalent* of this salt is 575·50; its composition may be expressed in symbols, 2 (Hg + Cy²) + (K + Cy).

100 grs. of *water*, at 60° F., dissolve 23 grs. The solution of the salt precipitates *a salt of lead*, white, perfectly *soluble* in an excess of the precipitants; if added to a *proto-salt of iron*, an orange-coloured precipitate takes place, which is evidently the *proto-cyanuret of iron;* for, on exposure to the air, it is readily converted into *Prussian blue;* if added to a *salt of lime*, a white precipitate falls which is *insoluble* in an excess of the precipitant; if added to solutions of *chloride of manganese, tartar emetic,* or *arsenious acid,* no precipitates take place : This salt is *slightly soluble* in *alcohol.*

II. *Hydrargyro-cyanuret of sodium* crystallizes from its solution in white, transparent *octohedrons.*

10 grs. of this salt were put into a small tube connected with another containing *chloride of calcium,* when the same process was employed as I have already mentioned under the preceding salt for ascertaining the water of crystallization; the difference between the weight of the tube containing the *chloride of calcium,* before and after the experiment, was 1·2 grs. of *water* = 8 equivalents of *water of crystallization.*

The decomposed salt remaining in the tube after the above experiment, was heated with muriatic acid; the solution of *chloride of sodium* obtained by this means was filtered, and the filter well washed; the solution and washings were evaporated very low, a small quantity of *sulphuric acid* was then added, and the whole evaporated to dryness in a weighed platinum crucible, and ignited, the same precautions being observed as were used in the preceding salt to prevent any *bi-sulphate* from being present; the weight of the *sulphate of soda* was 1·10 grs. = 0·77 grs. of *cyanuret of sodium* = 1 equivalent.

10 grs. of this salt (in crystals) were dissolved in *water*, and *sulphuret of sodium* was added to the solution, in the same manner as in the preceding salt; the *bi-sulphuret of mercury* thus obtained, after being well washed and dried on a weighed filter, weighed 7·03 grs. = 8·02 grs. of *bi-cyanuret of mercury* = 2 equivalents.

The filtered solution, consisting *merely* of *cyanuret of sodium*, was mixed with *nitrate of silver*, so that *the whole* of the *cyanogen* was precipitated as *cyanuret of silver,*

which, as in the former salt, was converted into *chloride of silver*, and weighed 11·52 grs. = 2·12 of *cyanogen* = 5 equivalents. 10 grains of this salt, therefore, contain,

Of *Water of crystallization,* 1·20 = 8 equivalents.
" *Cyanuret of sodium,* 0·77 = 1 equivalent.
" *Bi-cyannret of mercury,* 8·02 = 2 equivalents.
Loss, . . 0,01
—————
10,00

The equivalent of this salt (in crystals) is 631·25; the composition expressed in symbols is $2(Hg + Cy^2) + (Na + Cy)$.

III. *Hydrargyro-cyanuret of barium* crystallizes from its solution in large white transparent octohedrons, the angles truncated.

5 grains of this salt (in crystals) were heated in a tube connected with another weighed tube filled with fragments of *chloride of calcium;* the difference between the two weights was 0·9 grains of *water* = 23 equivalents of *water of crystallization.*

The decomposed salt remaining in the tube was acted on with muriatic acid; the solution of *chloride of barium* thus obtained was filtered, and a small quantity of *sulphuric acid* added, sufficient to precipitate the whole of the *baryta* as *sulphate of baryta,* which was placed on a weighed filter, thoroughly dried, and then weighed 1·00 = 0·81 of *cyanuret of barium* = 2 equivalents.

5 grains of the crystals of this salt were dissolved in *water,* to which a solution of *sulphuret of barium* was added; the *bi-sulphuret of mercury* thus obtained, after being well washed with distilled water on a weighed filter and dried, weighed 2·89 grs. = 3·14 grs. of *bi-cyanuret of mercury* = 3 equivalents.

The solution of *cyanuret of barium* left after the precipitation of the *bi-sulphuret of mercury* was mixed with a solution of *nitrate of silver* until *all* the *cyanogen* was precipitated as *cyanuret of silver,* which, as in the former analyses, was converted into *chloride of silver* by means of *muriatic acid;* after ignition short of fusion, it weighed 8·7 grs. = 8 equivalents of *cyanogen.* Therefore, 5 grains contain :

Of *Water of crystallization*,   0· 9 = 23 equivalents.
  ,,   *Cyanuret of barium*,   0·81 =   2 equivalents.
  ,,   *Bi-cyanuret of mercury*,   3·14 =   3 equivalents.
                  Loss, . .   0·15
                              ―――――
                                5·00

The equivalent of this salt (in crystals) is 1161·52. Its composition may be expressed in symbols by $3 (Hg + Cy^2) \times 2 (Ba \times Cy)$. 100 grains of *water* dissolve 17 grains of this salt; the solution, if added to a salt of *lead*, gives a white precipitate *completely soluble* in an excess of the precipitant; if added to a salt of *copper*, it produces, at first, a yellowish green precipitate, but, on the addition of an excess of the precipitant, the precipitate becomes white; with a solution of *tartar emetic*, it gives a white precipitate; with a solution of *chloride of manganese*, a white precipitate at first falls, which, however, very soon turns brown from the absorption of oxygen from the atmosphere; when added to a solution of *sulphate of zinc*, a white precipitate falls; with a *proto-salt of iron*, an orange-coloured precipitate takes place, which soon changes into *Prussian blue*, from the absorption of oxygen from the air; no precipitate takes place with *arsenious acid*.

This salt is dissolved *slightly* by *alcohol*. If a slight quantity of this salt be heated on a piece of platinum foil in the spirit lamp, it is at first decomposed, but directly after the decomposition has taken place, the edge of the platinum foil is surrounded with a *brilliant emerald green light*, owing to the phosphorescence of the *baryta*.

This salt loses just half its water of crystallization before it begins to be decomposed.

IV. *Hydrargyro-cyanuret of strontium* crystallizes from the mixed solutions of *bi-cyanuret of mercury* and *cyanuret of strontium*, in colourless transparent four-sided *prisms*.

10 grains (in crystals) of this salt were heated in a tube connected with another weighed tube filled with fragments of *chloride of calcium*; the difference between the weight of the tube before and after the experiment was 0·04 grs. This salt, therefore, contains no water of crystallization, but water mechanically mixed with it; when heated, the crystals decrepitate strongly.

The decomposed salt resulting from this experiment was treated in exactly the same manner as in the corresponding salt of *barium;* the *strontium* was here estimated as *sulphate of strontia,* which, after being thoroughly dried on a weighed filter, weighed 1·5 grs. = 1·1 grs. of *cyanuret of strontium* = 1 equivalent.

10 grs. (in crystals) of this salt were dissolved in *water,* to which a solution of *sulphuret of strontium* was added; the precipitate, after being well washed on a weighed filter with distilled water and dried, weighed 8·0 grs. of *bi-sulphuret of mercury* = 8·87 grs. of *bi-cyanuret of mercury* = 2 equivalents.

To the solution filtered from the *bi-sulphuret of mercury* (which consisted only of *cyanuret of strontium*) a solution of *nitrate of silver* was added, by which the whole of the *cyanogen* was precipitated as *cyanuret of silver,* which was converted into *chloride of silver* in the usual manner, the *chloride of silver* (after the necessary precautions had been observed to insure perfect dryness) weighed 11·4 grs. = 9·1 grs. of *cyanogen* = 5 equivalents. 10 grs. (in crystals) of this salt contain,

Of *Water,*	0·04	
„ *Cyanuret of strontium,*	1·10	= 1 equivalent.
„ *Bi-cyanuret of mercury,*	8·87	= 2 equivalents.
	10·01	

The equivalent of this salt is 579·76; its composition may be expressed in symbols thus, $2 (Hg + Cy^2) + (Sr + Cy)$.

100 grs. of distilled water, at the temperature of 60° F., dissolve 17 grs. of this salt. The solution precipitates a salt of *lead* white, which is insoluble in an excess of the precipitant; when added to a solution of *chloride of manganese,* a white precipitate falls, which quickly turns brown; with a *proto-salt of iron,* an orange precipitate falls, which is quickly changed into *Prussian blue;* it gives white precipitates with solutions of *sulphate of zinc,* and *tartar emetic;* the precipitate with the latter solution is completely soluble in an excess of the precipitant; the solution gives no precipitate with a solution of *arsenious·acid.*

V. *Hydrargyro-cyanuret of calcium* crystallizes in *octohedrons,* colourless and transparent, from the mixed solution.

12 grs. (in crystals) of this salt were heated in a tube connected with a weighed tube filled with fragments of *chloride of calcium*; the difference between the two weights of the tube was 0·64 grs. = 3 equivalents.

The decomposed salt was then heated with dilute muriatic acid; the solution, after being filtered and the filter well washed with distilled water, was mixed with a solution of *carbonate of ammonia* and boiled; the *lime* was thus precipitated as *carbonate*, which was placed on a weighed filter, well dried, and weighed 1·03 grs. = 0·96 grs. of *cyanuret of calcium* = 1 equivalent.

12 grains of this salt (in crystals) were dissolved in distilled water, to which a solution of *sulphuret of calcium* was added until the whole of the *mercury* was precipitated as *bi-sulphuret*, which, after being well washed on a weighed filter with distilled water, was thoroughly dried, and weighed 9·59 grs. = 10·42 grs. of *bi-cyanuret of mercury* = 2 equivalents.

To the solution filtered from the *bi-sulphuret of mercury* (consisting only of *cyanuret of calcium*) a solution of *nitrate of silver* was added; the *cyanuret of silver* thus obtained was converted into *chloride*, thoroughly dried, and weighed 14·60 grs. = 2·70 of *cyanogen* = 5 equivalents: Thus, then 12 grs. of this salt contain,

Of *Water of crystallization*,   0·64 = 3 equivalents.
,, *Cyanuret of calcium*,   .   0·96 = 1 equivalent.
,, *Bi-cyanuret of mercury*,   10·42 = 2 equivalents.
$$\overline{12·02}$$

The equivalent of this salt (in crystals) is 583·45.

The crystals decrepitate strongly when heated.

VI. *Hydrargyro-cyanuret of magnesium* crystallizes in small colourless transparent *octohedrons*, from the mixture of *bi-cyanuret of mercury* and *cyanuret of magnesium*.

10 grains (in crystals) of this salt were heated in a tube connected with another filled with *chloride of calcium*; the quantity of *water* which 10 grains were found to contain, by weighing the tube before and after the experiment, was 0·14 grs. = 1 equivalent of water of crystallization.

The *magnesia* contained in the decomposed salt left after the above experiment, was dissolved in very dilute sulphuric acid, the solution filtered, and the filter well washed with

distilled water; it was then evaporated to dryness in a weighed platinum capsule, ignited and weighed 0·86 grs. = 0·59 of *cyanuret of magnesium* = 1 equivalent.

10 grs. (in crystals) of the salt were dissolved in distilled water, to which a solution of *sulphuret of magnesium* was added; the *bi-sulphuret of mercury* thus obtained, after being well washed on a weighed filter, and dried, weighed 8·5 grs. = 9·27 of *bi-cyanuret of mercury* = 2 equivalents.

To the solution filtered from the precipitated *bi-sulphuret of mercury* (which consisted only of *cyanuret of magnesium*), a solution of *nitrate of silver* was added, which precipitated the *cyanogen* as *cyanuret of silver*, which was converted into *chloride* by acting on it with muriatic acid; the *chloride of silver*, after being completely dried, weighed 10·70 grs. = 1·97 of *cyanogen* = 5 equivalents; 10 grs., therefore, of this salt (in crystals) contain,

Of *Water of crystallization*, 0·14 = 1 equivalent.
„ *Cyanuret of magnesium*, 0·59 = 1 equivalent.
„ *Bi-cyanuret of mercury*, 9·27 = 2 equivalents.
              ⸻
              10·00

The equivalent of this salt (in crystals) is 557·65; its composition may be expressed in symbols thus, $2 (Hg + Cy^2) + (Mg + Cy) + (Aq.)$

The *simple cyanurets* used in forming the above salts were formed by adding a solution of *bi-cyanuret of mercury* to the corresponding *sulphurets*, when the *bi-cyanuret of mercury* exchanged its *cyanogen* for the *sulphur* of the *sulphurets*, and was precipitated as *bi-sulphuret of mercury;* the only *simple cyanuret* that was not made in this manner, was the *cyanuret of barium*, which was made by heating the *ferro-cyanuret* to decomposition and dissolving out the *cyanuret of barium* with distilled water.

The solutions of these salts have *no odour*, but a disagreeable metallic taste ; they are *not in the least decomposed* by exposure to the air; *mercury* can be detected in them all by *sulphuretted hydrogen;* a solution of *nitrate of silver* causes a white precipitate, which, I think, is most likely *not cyanuret of silver*, but a compound of *bi-cyanuret of mercury* and *cyanuret of silver;* on account of this, in analyzing the salts, they were converted into *simple cyanurets*

by the addition of corresponding *sulphurets*, previous to the precipitation of the *cyanogen* by a solution of *nitrate of silver;* for this method of estimating the *cyanogen*, I am indebted to Dr. Turner; most metallic salts are precipitated by the solutions of these salts; the crystals of these salts decrepitate strongly when heated, are decomposed and give off *cyanogen gas* together with *nitrogen;* all these salts crystallize in colourless, transparent *octohedrons*, except the *hydrargyro-cyanuret of strontium*, which crystallizes in 4-sided prisms.

I remain yours truly,

GEORGE HENRY JACKSON.

30, *Church-street, Spitalfields, March 26th,* 1836.

ARTICLE VI.

Experiments and Observations on Visible Vibration. By CHARLES TOMLINSON, ESQ.

(*Continued from* page 200.)

95. IN the details which have been already given respecting the nodal divisions of glass vessels during vibration, and the effects produced by those vibrations on the contained fluids, goblet-shaped glasses were, in most instances, the subject of experiment, but we have found that conical glasses, besides confirming most of the conclusions drawn from the phenomena of the first-mentioned vessels, afford facilities for observing many additional facts with respect to nodal division, and at the same time produce new and interesting sets of acoustical figures, some of which it is the object of the present paper to introduce and describe.

96. When a goblet is about two-thirds filled with water, (coloured water should be employed), and the fundamental note produced by means of a bow drawn against the edge, the nodes are indicated by the tranquillity of the water at certain equidistant points, while the centre of each vibrating arc becomes a centre of vibration, and a pleasing figure composed of fans 4, 6, or 8 in number, (and I now obtain as many as 10, or 12,) dependent on the dimensions of the vessel, but chiefly on the pitch of the note produced, is the

result; but if the glass be vibrated by passing the moist finger round its edge so as to produce the fundamental note, a system of four fans is produced, while the production of the secondary tones does not disturb the surface of the water; it is, therefore, to be considered that the results as stated in the present paper were obtained from glasses vibrated at one point of the edge by means of a fiddle bow.

97. In a passage from Dr. Young, already quoted, (10) it is stated, " that a vibrating glass, or bell, divides in general into four portions vibrating separately, and sometimes into six or eight." When a glass is producing a tone, whether fundamental or secondary, it must be considered not that the tone is produced by the aggregate or sum of four or more vibrating sectors, but that the same given note is produced by each and every sector of the glass, so that one part or sector would yield the note of the glass even though the other parts or sectors were damped or stopped altogether. If the sectors do not vibrate with exact isochronism, interference will, of course, result, (12), and it is to this nodal division that the peculiar softness, combined with richness and delicacy of notes produced from glass is probably due, for by it two purposes are served: *First,* Four nodal divisions, or systems, vibrating isochronously produce in every glass the fundamental tone; each division yielding the same note produces fullness and richness, by the combined vibrations of the four sectors; and, *Secondly,* in addition to the fundamental tone, secondary tones, amounting in large glasses to six or seven in number, and in smaller glasses to three or four are obtained, and the peculiarly high pitch of the upper secondary tones, surpassing that of any musical instrument, may be traced to nodal division, each secondary tone differing in this respect from the nodal division of the fundamental note, and without this difference secondary tones could not exist. Some of the secondary tones that have been obtained have been 6 or 7 octaves above the fundamental note, and, in one case, as many as 8 octaves, when a glass cylinder open at one end, 14 inches in length and 10 in diameter, was employed, the fundamental note of which resembled the lowest diapason stop of an organ. The various tones, therefore, of a glass are due to nodal division; each tone offering a different nodal arrangement, and each

sector of the glass contributes the same tone, as may be shewn by employing coloured water in the glass, and, during vibration, applying the finger to the three centres of vibration, as indicated by the fans; the 4th Sector will continue to vibrate the same note, but will be entirely divested of the fulness, richness, and intensity of the tone, contributed by the isochronous action of the four sectors. It requires some tact to perform this experiment success- fully; but, when performed, it supports the view we have taken of the production of the various tones. This result is also obtained by the spontaneous action of the glass itself, such a one as the cylinder above-mentioned. During the production of the notes dependent on 10 and 12 nodes, I have not been able to obtain a corresponding number of fans, the places where they ought to have appeared being vacant, and the water perfectly tranquil. This indicates a decided difference between the intensity of the vibration of different sectors of the same glass, where, in one case, out of 10 sectors only 7 vibrated with sufficient force to move the water; and, in the other case, out of 12 sectors, only 8 produced fans. The following figure will serve to shew the appearance of the water when the glass was yielding a very high secondary tone due to dodecagonal nodal division.

Fig. 1.
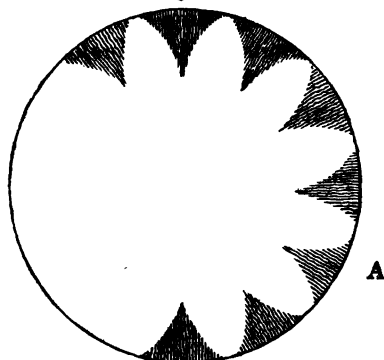
A, indicates the point at which the bow was applied; and it is singular, that on one side of it there are five fans, and on the other only two. This imperfect figure, it must be understood, was obtained from the large cylinder: from smaller vessels the 10 and 12 fans can be obtained perfect and without difficulty.

98. I have before stated, that the production of secondary tones from glass vessels is acquired by practice, the modus operandi being difficult to describe. I have, however, lately observed a method which is not only easy of description, but also of operation, when the bow, and not the moist finger, is the instrument employed for exciting vibration. When the glass is yielding, the fundamental tone (the result of 4 nodal divisions), if the finger be pressed upon a part of the periphery of the glass nearly opposite to that whereon the bow is acting, the note will suddenly pass to a secondary tone (the result of 6 or 8 divisions), and, by applying two fingers, the other secondary tones can be elicited. As soon as the desired secondary tone is produced, the finger, or fingers, should be removed; for the glass inclines to continue to vibrate the same note for every stroke of the bow, provided a second stroke is taken before the vibrations of the first have ceased.

99. The conical glasses (two in number) before alluded to, (95), were of the following dimensions :—Perpendicular depth 3½ inches, diameter at the mouth 4 inches.

Besides these, several conical wine glasses of green glass, and common chemical test glasses, were employed. Each one always yielded one fundamental and two or more secondary tones, both when empty and when filled about two-thirds with water.

The two largest glasses, which I shall designate, First and Second, yielded the following results:—

<div align="center">FIRST GLASS WHEN EMPTY.</div>

E middle octave of flute. Fundamental note,
B third octave.   ⎱
F sharp fourth octave.   ⎰ Secondary tones.

Besides these, another secondary tone was produced, which, from its high pitch, I could not determine, but which I believe to be in the fifth octave.

<div align="center">FIRST GLASS CONTAINING WATER.</div>

C sharp middle octave,   ⎱ Fundamental note producing
or second C of flute.   ⎰ 4 nodes as indicated by 4 fans.

A sharp third octave.   ⎱ Secondary tone.
                         ⎰ Six nodes.

F sharp fourth octave. } Secondary tone. Eight nodes.

A note in the fifth octave. } Secondary tone. Ten nodes.

SECOND GLASS WHEN EMPTY.

E middle octave.   Fundamental note.

A third octave. } Secondary tones.
E fourth octave. }

Besides these, two other tones, which, on account of their extremely high pitch, I have not been able to determine, but am disposed for circumstances to be stated hereafter to place in the fifth and sixth octave.

SECOND GLASS CONTAINING WATER.

C middle octave. } Fundamental note. Four nodes.

G third octave. } Secondary tone. Six nodes.

E flat fourth octave. } Secondary tone. Eight nodes.

A note in the fifth octave. } Secondary tone. Ten nodes.

A note in the sixth octave. } Secondary tone. Twelve nodes.

From the foregoing statement of results obtained from the two conical glasses, and which have been abundantly confirmed by other glasses of various dimensions and forms, I conclude, that the lowest or fundamental note of *every* glass is the result of a quadripartite system of nodal vibration; that the ascending secondary tones are due to an increasing number of nodes and vibrating sectors, and that the increase is constant for every octave by two, and it will probably be found hereafter, that the number of nodes as indicated by water vibrating in glass and other vessels will shew at once the octave of the note with reference to the fundamental note of the glass whence it is produced, and that this fundamental note has no reference to the same note on a musical instrument in point of octave or pitch, for it is obvious, that small vessels yield acute tones, and large vessels on the contrary yield grave tones; but whether

any particular tone be accute or grave, provided it be
the lowest in point of pitch, that can be obtained from
the glass, such a tone is the fundamental note of the glass,
and is due to a quadripartite system of vibration. In ascend-
ing in the scale, the first secondary tone divides the glass
into six parts, the second secondary tone into eight parts,
and so on; and this, I believe, will be found true of all
vessels of glass of whatever dimensions. I believe it will
also apply to vessels of porcelain and crockery ware, as
well as of metal; but this will require a separate paper for
investigation.

100. The centres of vibration are indicated by the longest
lines which dart from certain equi-distant parts of the inner
circumference of the glass on the liquid surface towards the
centre, which lines decrease in length on either side of the
centre of vibration until they cease to be perceptible (see
figure 1); and the nodes are shewn by the tranquillity of
the water between any two centres of vibration. When a
glass contains water, and the bow is strongly drawn against
its edge, the water at and about the centres of vibration
will be elevated, and at the nodes, the water will be de-
pressed; and when the vibrating action has ceased, and the
surface of the water become tranquil, these elevations and
depressions will be exhibited in the form of a curved line
passing round the interior surface of the glass and above
the surface of the water. If the action of the bow be strong
the water will be sprinkled about on the interior surface of
the glass above the liquid surface, and this sprinkling will
exhibit the curved line very perfectly. Figure 2 will
illustrate this.

Fig. 2.
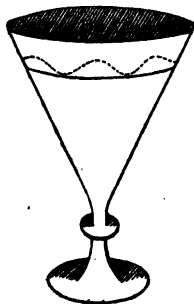
The liquid should be carefully poured so that the glass above the liquid surface be preserved dry, the portion of the glass between the edge and the curved line will then be seen to be partially sprinkled, but between the level of the water and the curved line, it becomes wholly-wetted thereby, indicating the height to which the fluid had been thrown at the centre of each vibrating arc. The number of undulations in this curve depends on the number of nodes, and both depend on the note produced.

(*To be continued.*)

ARTICLE VI.

The Art of Dyeing.

THE spots which strong solutions of chloride of lime produce are thoroughly bleached.* Weaker solutions act by

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* There are several other methods of testing chloride of lime; 1. by determining how much of a solution of indigo in sulphuric acid is decolourized by a given weight of the powder. 2. By means of muriate of manganese, which is thrown down by the bleaching-powder in the state of teroxide; the weight of teroxide obtained measures the strength of the powder. 3. By the addition of a solution of bleaching-powder to a solution of proto-sulphate of iron, till the smell of chlorine begins to be perceived. 4. By means of a solution of arsenious acid, according to Gay Lussac. The weight of the chloride to be tested may be fixed at 154·38 grs.; and this may be dissolved in water in such a manner that the solution, including the deposit, shall be equal to 1·76 pint. If we take a constant volume of this solution as ·61 cubic inch, divided into 100 parts, and pour a solution of arsenious acid in muriatic acid gradually into it; measured out in the same way until the chlorine be neutralized, the strength of the chloride will be proportional to the number of parts of the solution of arsenious acid which the chloride required. If the chloride destroyed 100 parts of arsenious acid solution, then the value of the chloride would be 100°; if only 80, then its value would be 80°.— *Ann. de Chim.* lx. 225. 5. Ferrocyanodide of potassium may be substituted for the arsenious acid. This method is due to Gay Lussac, although it has been known in this country for some time as the discovery of an Italian instrument-maker. The solution of this salt has little or no action upon the chloride, until it is rendered acid. To detect the point of saturation, a drop of a solution of indigo is added to the saline solution, which assumes a fine green colour. This colour fades in proportion as the indigo is destroyed, by the progress to saturation.— *Ann. de Chim.*, ib. 6. The proto-nitrate of mercury, when mixed with a solution of common salt, or with muriatic acid, affords a white precipitate of muriate of mercury, which disappears completely, and is changed into chloride by the addition of a solution of chlorine, or chloride of lime, provided the mixture contains a sufficient quantity of free acid to saturate the base of the muriate.—*Ann. de Chim.*, ib.—EDIT.

preference only on the circumference, so that in very weak solutions the middle remains unchanged, and the action is only shewn on the borders.

It should be remarked, that the solution of chloride of lime should not be brought upon the dyed cotton before it has become clear, which soon takes place. In no case should it be filtered, as it weakens the chloride of lime, and destroys the filtering-paper.

If a piece of calico is moistened with a clear solution of chloride of lime, and hung up in a room, a strong odour of chlorine will soon be perceived, and the calico in a short time becomes brittle, and can be torn to pieces.

The same destructive effect is produced by the chloride of lime upon calico which is allowed to remain for some time in a solution of chloride of lime.

A clear solution of chloride of lime acts more injuriously upon cotton than one which is milky, and contains an excess of lime. Hence, the clear solution of the chloride of lime destroys colours, which the milky solution leaves unaltered.

This property is important in the manufacture of madder-purple, where the solution of chloride of lime is employed to prepare on the purple, white, blue and yellow, &c., patterns. The madder-purple is printed with mordants, which decompose the chloride of lime, and set the chlorine free, which bleaches during its disengagement. On the other hand, the unprinted portions retain their red colour, unless when the solution of chloride of lime contains no excess of lime. The lime is also useful by saturating the chlorine set at liberty. In the opposite case, instead of a regular edged pattern, the borders will be irregular.

It has been already stated, under the tests for chloride of lime, that it bleaches many colours when locally applied. The following solution may be employed for printing it on cloth:—1 lb. chloride of lime in 30 to 40 lbs water. It is very difficult, however, to thicken such a solution.

To dissolve the chloride of lime, much water must not be at once poured upon it. It should first be mixed into a magma, with double its weight of water; the large pieces should be broken by means of a wooden pestle, and allowed

to stand for twelve hours. Then the necessary quantity of water should be added. If less water is employed, a thick mass is formed, which mixes and dissolves with difficulty in water.

Chloride of lime is employed for bleaching calico. In order that it may be employed with sufficient advantage, all the soluble leys and acids of the calico should be removed by water. As chloride of lime acts first upon foreign matter attached to the fibre before it bleaches it, a considerable loss of chloride will be experienced, if the cotton contains weavers' glue. The alternate treatment of linen with chloride of lime and sulphuric acid is injurious, but has no bad effect upon cotton. A more certain method is by the employment of chlorine without the addition of acid; and it is still better when it is united with potash or soda, and when the bleaching solution contains an excess of the same. Such a solution may be formed in the following manner from that of chloride of lime. Dissolve 20 lbs. of chloride of lime in 200 lbs. water, and mix them with a solution of 24 lbs. Glauber salts in 100 lbs. water. When the mixture has become clear, the clear solution should be removed from the sediment, and 1 to 2 lbs. potash dissolved in water added to it.

This solution (chloride of soda) acts, when diluted with water, slowly as a bleaching substance, but with certainty, and sinks deep into the fibre.

When solutions of chlorine are kept in wood, they corrode the latter, and lose their bleaching power. This may be prevented by a coating of two parts wax to one colophonium.

Bran is employed to cleanse prints and dyed stuffs, and also as an addition to the dye solution.

Bran for cleansing Prints.—It will be shewn, when treating of cow-dung, that mordants for delicate colours, as yellow, pink, lilac, &c., cannot stand cleansing with cow-dung. Bran can be employed in these cases with advantage. It has a similar action to the cow-dung, in so far that it renders insoluble, and precipitates those parts of the mordant which are not intimately combined with the cotton, by which they will be prevented from collecting in those parts which should remain white.

The quantity of bran to be added depends upon the weakness or strength of the mordant. In the first case, the quantity of bran should be equal to the weight of the calico ; in the latter case, more is required.

Bran for cleansing dyed stuffs.—Bran is employed to cleanse white grounds, especially such as are dyed with madder. It removes the colours which have run into the unprinted parts, and imparts to the madder-red a clearer lustre.

The bran-bath can, with madder colours, be brought to the boiling point, but must not long remain there, otherwise the colours will be weakened.

Soap may be added to the bran-bath, especially where there is much red, and less of white ground.

Upon white grounds dyed with quercitron, bran acts to great advantage ; only the yellow colours will be injured if the bath is too hot. The temperature should never rise above 189° F.

In all these cases portions of the bran combine with the calico, and the colours upon it. In consequence, the colours become brighter, and the calico acquires stiffness after careful washing.

Bran as an addition to the dyeing solution.—Logwood colours appear brighter when a determinate quantity of bran is added to the solution, and the dyeing performed. This is particularly important in pieces upon which the alum or copper mordants have been printed.

COLOURS WITH LOGWOOD AND ALUM MORDANT, NO. I.

When 12 lbs. cotton are dyed with 1 lb. logwood and 3 lbs. bran, the colour produced is a fine violet blue ; without the bran, the colour is brown.

The wood and bran are first boiled together with little water, by which the solution acquires a bright-red colour; it is then cooled by the addition of more water, and the previously well-moistened cloth introduced.

The dyeing is performed at a heat which ascends to boiling. After the dyeing, the solution to which bran was added possesses a yellow colour. It still contains a sufficient quantity of logwood to dye 3 lbs. of cloth violet blue.

COLOURS WITH LOGWOOD AND COPPER MORDANT, NO. I.

With 10 lbs. cloth, 1 lb. logwood, and 3 lbs. bran, the resulting colour is clear light-blue; without the bran, it is a dirty grayish-blue. The process is the same as with the alum mordant.

With madder-red and alum mordant, the action of the bran is still more striking. It gives not only the red a clearer colour, but it also increases the dyeing powers of the madder; since, with the same quantity of madder, by the addition of bran, more cloth can be dyed than without the bran.

With 12 lbs. Avignon madder, 36 lbs. bran, and 9 lbs. cloth, the colours in each of three successive emersions in the vat become clearer and lighter red; while without the bran, the first emersion gives a very dark red, and the third a light reddish yellow colour. The result shews that the action of the bran consists in producing an uniform distribution of the colouring matter of the madder over the cotton.

The proportion of the bran to the madder as here given (36 to 12) is the best. If less is taken, the madder colour is not so dark; if more is employed, the same result follows—a pale-red only is obtained.

The dyeing should be completed at the boiling temperature. The addition of bran to madder on a white ground is very useful. If wheat-bran cannot be had, rye-bran may be employed.

The action of bran when added to solution of Fernambuc is not less remarkable. The colour obtained is a clearer red, than when it is dyed without bran.

Bran thickens the solution much. This is an obstacle to the dyer, as it compels him to employ more water than usual, and only allows half as much cloth to be placed in the vessel for dyeing at once, as formerly. This difficulty may be removed by employing a decoction of the bran, instead of the bran in substance; the former possesses the same action as the latter.

The decoction is formed by boiling the bran with a sufficient quantity of water, to form a thin paste. From this the watery portion is strained; the residue is boiled, and again strained. The residual bran may be used for fodder.

The decoction of bran speedily sours; it should not, therefore, be prepared long before using it. It often sours during dyeing, especially when the process is long. This is very injurious to the dye; it is prevented by the addition of chalk, in the proportion of 20 lbs. of bran to 1 lb. of chalk.

The bran which has been employed for dyeing with madder acquires a reddish-brown colour; and when boiled with spirit of wine, gives up this colouring matter. The quantity of this is, however, so inconsiderable, that the loss will be far outweighed by the favourable action of the bran.

Cow-dung.—The cow-dung bath has been long employed, and certainly with great propriety, as a method for cleansing the printed cotton. Its principal use is to remove the superfluous quantity of mordants, and the substance employed for thickening, which do not adhere to the cotton, and thus prevent them from precipitating upon those portions which are unprinted, and are intended to remain white.

To have a clear notion of the action of cow-dung, it is necessary to be acquainted with its constituents, or those substances which are principally necessary for the object mentioned.

The active matter of cow-dung can be separated by sulphate of copper, and its properties ascertained separately.

Let fresh cow-dung be agitated with twenty times its weight of water, and filtered through fine paper. Let the clear dark-brown solution which passes through, be mixed with a solution of sulphate of copper; a dark-brown precipitate results, which, after washing with water, will be decomposed by sulphuretted hydrogen. This separates the copper contained in the precipitate in the form of sulphuret of copper, and a clear brown solution is obtained, which reddens litmus strongly; and, when evaporated, leaves a dry brown mass, which tastes acid and astringent. This mass, which consists of a brown colouring matter, and a very strong colourless acid (*Bosoprine* and *Bosopric* acid[*]), is, therefore, the efficient means of purifying the mordanted

[*] I have substituted these terms for the cow-dung brown, and cow-dung acid of the original, from βȣ̃ς and κοπρος, Vaccae fimus.—EDIT.

cotton in the cow-dung bath. If it is dissolved in water, and mixed with solutions of alumina, iron, copper, or tin mordants, brown precipitates are formed, which are nothing but combinations of *bosopric acid* and *bosoprine*, with alumina, iron, copper, and tin. The same compounds are formed when calico, printed with these mordants, is boiled with it. The *bosopric* acid and *bosoprine* take the place of the acids which were united with the mordants on the calico. Hence, when calico printed with acetate of alumina mordant is boiled in the mixture described, of *bosopric acid* and *bosoprine*, the acetic acid separates from the alumina, and both the constituents of cow-dung combine in its place, and form *bosoprate* of alumina, united with *bosoprine*, which are both insoluble in water; and, therefore, remain in combination with the calico, which now acquires a brown colour. The other mordants act in the same manner. Acetate of iron, for example, printed upon calico, will be converted into *bosoprate* of iron.

When cow-dung itself is employed instead of the two constituents, the same combinations are formed upon the calico, while the remaining elements of the cow-dung have no action upon the mordants. Hence, the effect of passing the mordanted calico through a hot cow-dung bath is nothing else than actual dyeing; so that in applying the subsequent dyes, the combinations of the cow-dung colours must be again destroyed, in order that the new colours may be substituted.

All colouring matters do not produce this decomposition in the same degree; madder effects it most completely; therefore, goods dyed with madder-colours are always cleansed with cow-dung; cochineal, on the contrary, possesses the least effect; a specimen dyed with alum mordant, No. 1, and cochineal-red, shews that the colouring matter of the cochineal cannot separate the colour of the cow-dung from the alumina, but that it has combined with both, and consists of a mixture of brown and red.

With iron mordant this occurs in an equally remarkable manner. The cotton is mordanted by simply dipping it in a solution of iron alum, and washing it immediately after in water. If one of the specimens is boiled with cow-dung,

and both are dyed with cochineal, a cochineal-gray is obtained, much lighter with cow-dung than without it.

In pink, yellow, lilac, and in general all light colours, the employment of cow-dung should be avoided. In these cases bran should be used.

It has been already stated, that *bosopric acid* and *bosoprine*, when mixed with the aluminous and iron mordants, precipitate these, and form, with the alumina and iron, compounds insoluble in water; the same occurs with an aqueous extract of cow-dung. This property explains the use of cow-dung in cleansing white grounds.

When calico is printed with a mordant consisting of acetate of alumina thickened with starch, more mordant is deposited on the cotton than can be retained by it in the subsequent washing. This excess dissolves in the water, which is converted into a weak solution of acetate of alumina; this acetate of alumina combines only with the unprinted parts which should remain white, and mordants them, so that they are likewise dyed.

If cow-dung on the other hand is employed, no acetate of alumina will be dissolved in the water; but as soon as it separates from the calico, it will be seized on by the *bosopric acid*, rendered insoluble, and thus inactive.

With the acetate of iron mordant, the action is exactly the same.

When *bosopric* acid is boiled with chalk, *bosoprate* of lime is formed, which is insoluble in water, and acts like cow-dung.

This property enables the dyer to add chalk to the cow-dung bath, which is very necessary in many cases. When many printed pieces are passed through the cow-dung bath, they become very acid, from the acetic acid which was contained in the alumina or iron mordant. This would prove injurious to the pattern, but is prevented by the addition of chalk, which combines with the acetic acid and neutralizes it.

By the addition of lime the same object is obtained, but the colours are less clear and lively.

Soap-suds produce a favourable change on a number of colours, which is called *clearing*.* A portion of the alkaline

* See Records of General Science, i. 169.—EDIT.

salt of soap combines with the colouring matter upon the calico when the dyed cloth is passed through warm or hot soap-suds, and renders the colour darker. At the same time, some oil, or fat, goes to the mordant, by which most colours acquire more permanence. The great durability of Turkey-red, in the manufacture of which olive-oil is employed, is to be ascribed to this cause.

In clearing the dyes by soap-suds, care must be taken not to dissolve the soap in water containing lime, otherwise a combination of soap and lime is formed, which is deposited on the cloth, in the form of a fine white powder, rendering it ugly, and injuring it.

If no water but such as contains lime can be had, the objection mentioned may be removed by boiling the solution, and skimming off the combination of lime and soap from the surface.

Soap-suds are also employed to cleanse white grounds, where the property of the soap-suds already mentioned is exhibited, of making many colours more permanent.

If we take two pieces of mordanted cotton, boil them both in the same solution of madder and wash them; they have both acquired a reddish colour. If one of these pieces is passed through hot soap-suds—well washed and laid with the other piece on the grass to bleach—it will be found that one of them will be bleached completely white, the other, on the contrary, much slower. The last, is that which has been passed through the soap.

Hence, it follows, that madder colours on a white ground should not be passed through soap-suds before the bleaching is completed. In this case, a bran bath may be employed with advantage, instead of the soap-suds.

From the beneficial action of oil in rendering the colour of Turkey-red permanent, it might be concluded, that the process of passing the mordanted calico through soap-suds before dyeing it with madder, would be beneficial, as also in this case, a combination is formed with the oil, mordant and cotton. The dye, however, is not improved. It appears faint, and can be scraped off even when carefully dyed with madder. This shews, that in the Turkey-red, the oil exists on the calico in quite a different state from that which it possesses, in the case of the soap.

Bleaching the calico.—According as the calico is intended for block and cylinder printing, or for a white ground, the bleaching varies.

In calico for block and cylinder printing, the bleaching does not require to reach the internal parts of the fibre; a superficial whiteness is sufficient. It is sufficient, therefore, to cleanse the cloth by means of warm bran water and yest, in order to remove the weaver's dressing; then alternately to boil it with ley, and to bring it into a sulphuric acid bath, and lastly to lay it out in the sun.

For white grounds, it is necessary to carry the bleaching into the internal parts of the threads, Otherwise the colours are forced into the ground and bleached spots are produced. When, therefore, the cloth has been prepared by cleansing and alternate treatment with ley and acid, the bleaching in the sun should be combined with the use of chloride of lime and chloride of soda. The cloth should be placed in very weak solutions of these.

To remove the colours which are deposited upon improper places, as on white grounds, bran and soap-suds are employed alternately with the action of the sun. The necessary process is given under soap-suds.

In madder colours with a white ground, chloride of soda assists powerfully, in making the madder-red more red, and the white more white.

For the purpose mentioned, the chloride of soda may also be employed when mixed with soap-suds. This mixture gives the madder-red more lustre.—(See Turkey-red manufacture). Chloride of soda must be employed in a very dilute state. The degree of dilution depends upon the depth of colour, and must be determined by trial. When the chloride of soda is too strong, it acts too rapidly, and imparts to the cotton an appearance, as if a white powder was laid over it. This proceeds from the fine threads being bleached white before the chlorine has had time to act upon the twisted threads.

Application of mordants to calico.—This process constitutes the basis of the dyeing of calico. It depends on the circumstance, that certain earths and metals when they are dissolved in water by the assistance of acids, combine intimately with the cotton fibre, and that these combinations

are still in a condition to unite with a third body, viz., the colouring matter.

For this purpose, such solutions or mordants are employed as combine readily with the cotton, when moistened with them, and are not again removed by washing in water. Even by boiling with water, the mordants should remain in combination with the fibre. The mordants must not merely cover the fibres, they must penetrate into them and be intimately united. When this is the case, the colours appear saturated and clear.

Alumina, iron and tin salts possess these properties in the strongest degree.

Their application is two-fold, according as light or dark colours are wanted.

The process of mordanting for light colours, (light, colourless grounds) is performed by moistening the calico with very dilute mordants, and immediately afterwards washing it in water. The mordant is contained in a trough, in which there is a wooden cylinder, which is completely covered by the mordant. The calico is passed under the cylinder, and between two wooden rollers, and pressed; then rolled upon a third roller, and washed. As it produces a considerable difference whether the calico is a long or short time in contact with the mordant; the greatest uniformity is obtained by allowing the calico to pass twice through the same mordant. In this case, the first untouched end comes last the second time, and is thus equalized. Many solutions of mordants are completely exhausted by calico, so as to leave only water behind ; it is, therefore, to be observed, that mordants become weaker in proportion to the number of pieces which come in contact with them. Fresh mordants must, therefore, be added occasionally. The application of mordants for dark colours (heavy colourless grounds) is produced by impregnating the calico with very strong mordants, and then drying it. The impregnation is produced by means of the rollers already described.

The drying requires much caution when the grounds are uniformly produced; it must be done as quickly as possible, in order that the mordant may not stick to some places more than others. This will be avoided by passing

the calico over copper cylinders, which are heated by the vapour of water, or, what is better, by spreading it out on a very hot drying-stove, between wooden cylinders, until it is dried.

Calico which is impregnated with the acetate of alumina mordant, should be hung up in an airy place for six or eight days. More acetic acid is disengaged in this case, and the colours become more saturated.

When many pieces hang in one place where there is no current of air, the disengaged acetic acid will prove injurious to the mordant; it is, therefore, necessary to obtain a change of air by proper ventilation. It is still a better plan to saturate the acetic acid by disengaging ammoniacal gas. This will, in most cases, however, be too expensive.

Calico dyeing.—During the dyeing, the mordanted calico (as has been stated under water) has to overcome the dissolving power of the water, in order to abstract the colouring matter from it. This is effected generally incompletely, so that the solution, even when the calico to be dyed is so considerable that it can only be half-saturated, still retains some colouring matter. This can only be taken up by boiling with fresh mordanted calico; yet still some colouring matter often remains, which can only be exhausted by a third piece of cotton.

This remarkable property depends on the circumstance, that the dye contains many colouring matters which are not equally taken up by the calico. In this case, one after the other combines with the mordanted calico. This happens particularly with madder. If, in a solution which contains 4 loths (1·187 oz.) of madder, calico mordanted with the alum mordant, No. 1, be dyed three times in succession, the cotton in the first dyeing will be dark-red, from the madder-red; on the other hand, that of the third dyeing will be yellowish-red, from madder-yellow. The calico from the second dyeing possesses an intermediate colour.

If the same experiment is made with 4 loths (1·872 oz.) of quercitron and mordanted calico, a similar result is obtained. The pieces of calico which were dyed in succession come out at first dark-yellow, from the solution; they amount to 14 loths (6·55 oz.) A dark-yellow is no longer

obtained; but the pieces which are likewise dyed in succession come out light-yellow, and amount in weight only to 10 loths (4·68 oz.). Quercitron bark, therefore, contains one colouring matter which dyes dark-yellow, and another which gives a light-yellow colour.

Hence, it follows, that a dyeing matter can be deprived of its whole colouring matter only by repeated dyeing. Some solutions contain still a quantity of colouring matter in which very saturated and dark colours will be produced, which are mostly formed by an excess of colouring matter. That this colouring matter may not be lost, other pieces destined to be dyed of the same colour may be dyed in these solutions, and may afterwards, in a new solution, receive the proper saturation.

As the dyeing materials, madder, logwood, fernambuc, &c., are to be considered as fibres of plants which, by boiling in water and in dyeing the calico, lose their colour, so the action of these vegetable fibres upon the colouring matter combined with them, requires some attention. Most of them retain their colouring matter so strongly, that it is impossible to deprive them of it by pure water alone. Thus, logwood may be boiled from 20 to 30 times, with 6 times its quantity of distilled water, without being completely taken up, and the remaining fibre gives a reddish-brown colour to potash ley.

Calico, impregnated with the alum mordant, takes up the colour more rapidly with the assistance of water. It takes immediately from the water the dissolved colouring matter, and converts it into pure water, which can again dissolve new colouring matter, which the calico again takes up.

In this way, a peculiar exchange frequently takes place. If the mordanted calico is not completely deprived of all uncombined superfluous mordant, or if the dyeing solution possesses the property of taking up the mordant from the calico and dissolving it, the coloured fibre of the dyeing material is mordanted, while the mordanted cotton fibre is dyed.

Both fibres divide themselves in the mordant and in the dye, to the great annoyance of the dyer. Hence, the madder employed for the Turkey-red dye is almost as dark

as the calico itself; since it is impossible to purify the pieces on a large scale, in such a complete manner as to prevent any more of the mordant from passing away.

This is managed during the preparations for dyeing. In madder-dyeing, a cheap or inferior kind of madder is employed, or galls or tan may be used, which renders the mordant more permanent on the calico, by forming gallate and tannate of alumina, which take up the uncombined mordant.

The operation of passing the cloth through a cow-dung bath is a similar preparation for dyeing.

In most cases, it is most economical to employ for preparation the same colour which is to produce the dye, or a substance which does not dye, as the excrescence on the birch.

This principle, that the extraction of colouring matter constitutes a discoloration of the fibres of plants which hold their colouring matter in chemical combination, explains the reason why dyeing cannot be well effected with the fresh or green parts of plants. Here the fibres and colouring matter of plants, are less intimately combined; they are still in their natural living state, but the latter will be taken up completely at a boiling temperature. This kills the plants, and puts them in the condition which would be produced in another way by drying.

In general, the dyeing commences when the solution is lukewarm, and terminates when it is boiling. With colouring matters which have a great tendency to unite with the mordanted calico, as logwood, it is proper to place the cloth in the cold solution at 50° or 54°$\frac{1}{2}$; otherwise the cloth will be spotted or unequally dyed.

In general, slow heating of the solution is an important object for obtaining an equal colour ; therefore, steam-heat is to be preferred, as, by opening or closing the steam-pipe, it is easy to increase or diminish the heat.

Test of dyeing on a small scale, and mode of estimating the necessary quantity of colouring matter to be used in dyeing.— It is impossible for any one to learn in a dye-work thoroughly the art of dyeing ; this must be attained by trials upon a small scale, which will give a regular measure for

trials upon a large scale, where the quantities are only esti-
mated by the weight in the hand. It is astonishing how
little this has hitherto been attended to, and yet there is
nothing of greater importance in a dye-work.

The proportion of the dyeing materials to the cloth to be
dyed is especially to be attended to; the latter must never be
estimated by the volume, but by the weight. With the
same madder and the same cloth, a red, or a brownish-red,
is produced, according as the same, or a double quantity, is
employed for dyeing. Thus, logwood gives with the same
mordant a lilac and a blackish-blue, when it is employed in
small or great proportion.

The same rule holds with other colouring matters; and
the shade of the colour required can never be estimated
with accuracy, when the proportionate weight of the cloth
to be dyed is not known. Hence, often nothing at all is
learned from large and prolix treatises upon new colouring
matters, as this important point is omitted ; and the dyer,
who has not accustomed himself to make trials accurately
with grains or half ounces, but has been in the habit of em-
ploying pounds, and obtaining the same results on a large
scale, will require to be content with many spoiled pieces.

When the dyer approaches the point where he obtains
the darkest colours with the smallest quantity of dye, it is
important to know its dyeing power; this is effected in the
following manner :—

A piece of calico, impregnated with No. 1. mordant, and
well washed, should be cut into equal portions of about two
square inches each. Weigh out, then, $\frac{1}{4}$, $\frac{1}{8}$, or $\frac{1}{16}$ of a loth
(·468 oz.), or even less; about ten to five grains of the co-
louring matter to be tested; place it, with the requisite
quantity of water, in a porcelain dish over a spirit of wine
lamp, and dye the pieces of calico in succession until the
solution is exhausted, and the calico takes up no more co-
lour. Each bit of dyed calico should be first rinsed in a
little water, and this water should be again added to the
original solution, so as to lose no colouring matter. The
dyed pieces are then to be laid together when they are dried,
and those are to be picked out which are equally saturated,
and weighed. The weight gives the quantity of mordanted

calico which the given quantity of the colouring matter can dye.

As, however, the portions of calico dyed first are often, as has been stated, super-saturated with colouring matter, a second trial is necessary, which consists in dyeing as much mordanted calico as before, with the addition of $\frac{1}{3}$, $\frac{1}{4}$, or $\frac{1}{8}$ more, but all at once. When this result is compared with the former, the necessary quantity of colouring matter will be readily determined, without having recourse to a third experiment.

These distinctions are only available for calico impregnated with the alum mordant No. 1. For iron and mixed mordants they must be re-arranged, since these combine in other proportions with the cotton fibre from the alum mordant, and require, therefore, different quantities of colouring matter for saturation, or for producing the requisite shade.

(To be continued.)

ARTICLE VIII.

ANALYSES OF BOOKS.

I.—*Der Angehunde Botaniker oder kurzer und leichtfaszliche anleitung, die Pflanzen ohne Veihulfe eines lehrers kennen und bestimmen zu lernen von T. A. F. Schmidt, Ilmenau,* 1834.

THE first chapter describes the organs of plants. The 2nd, 3rd, 4th, and 5th, treat of the root, stem and bark; the 6th and 7th, the anatomical structure of plants; the 8th to 17th, are devoted to the description of the flowers with their different parts; the 18th to the 21st, treat of the fruit and seeds; the 22nd to the 28th, make us acquainted with the different botanical systems, as those of Linneus, Jussieu and Reichenbach.

The two first are sufficiently well known; but an acquaintance with that of Reichenbach is not so generally diffused. His system is founded on the developement of the life of plants, or on the metamorphoses of plants. According to his view, the life of plants may be divided into two distinct periods, viz., a period anterior to growth, (*Vorleben*) when the plant is in the form of a seed-bud, and the second period exists in perfection, when the plant becomes possessed of a stem and flower. He divides plants into eight classes.

A. Class Inophyta, fibrous plants, which possess the power of living in the earth, and are independent of light.

I. Plants with shoots, (Keimpflanzen) 1 class *Fungi*, (order 1) *Gymmomycetes*, Formation I. Blastomycetes, (Shooting fungi) 1. Uredinei, 2. Tubercularei. Formation II. Hyphomycetes, (*Fibrous*

fungi), 3. Byssacei, 4. Mucedinei. (Order II.) *Dermadomycetes.* Formation I. Gasteromycetes, (Cuticular fungi), 5. Sclerotiacei, 6. Lycoperdacei, 7. Sphaeriacei. Formation II. Hymenomycetes, 8. Tremellini, 9. Morchellini, 10. Hymenini.

This is sufficient to give a general idea of his minute division of this class of plants.

II. Budding plants, (*Knospenpflanzen*). Plants with shoots and buds.

2. Class Lichens, and Psoræ, order 1. *Gymnosporæ.* 2. *Ascopsoræ.*

B. Coriophyta, or plants which require light. Endogeneæ. 1. Root plants. III. class, Chlorophyta, (absorbing plants). Order 1. *Algæ.* 2. *Musci.* 3. *Filices.* 2 stalked plants, IV. class, Acroblastæ. Order I. *Rhizo-acroblastæ*, including Isoeteæ, Potamogetoneæ, Aroideæ, &c.

2. *Caulo-Acroblastæ*, Gramineæ, Commellinaceæ, Irideæ.

3. *Phyllo-Acroblastæ*, Liliaceæ, Palmaceæ.

Amphigeneæ, or Cotyledoneæ. I. Apetalae. V. class, Synchlamideæ. Order I. Enerviæ, including Nayadeæ and Imbricatæ. 2. *Rigidifoliæ*, Equisetaceæ, Proteaceæ, &c.

3. *Venosæ*, Urticeæ, Piperaceæ, Laurineæ, &c.

II. Monopotelæ. VI. class, Synpetalæ. 1. *Fissifloræ*, Rubiaceæ, Compositæ, Campanulaceæ, &c. 2. *Lobifloræ* Labiatæ, Asperifoliæ, Solaneæ, Polygaleæ, &c. 3. *Rotifloræ*, Lysimachiæ, Primulaceæ, Ericeæ, Asclepideæ, &c.

III. Flower plants. VII. class, Calycanthæ. 1. Leguminosæ and Parvifloræ, including Umbelliferæ, Rhamneæ. 2. *Confrines* Rotifloræ, Corniculatæ, Loasaceæ. 3. *Concinuæ*, Onagrareæ, Myrtifloræ. 4. Fruit plants. VIII. class, Thalamanthæ.

1. *Thylachocarpiæ*, Crucifloræ, Cistifloræ. 2. *Schizocarpicæ*, Ranunculifloræ, Geranifloræ. 3. *Idiocarpicæ*, Tilifloræ, Aurantiifloræ.

The 26th chapter treats of the analytical method and the system of Lamarck. The 27th chapter contains remarks on Cryptogamia; and the 28th *describes the method* of preparing a herbarium.

II. *Die Mineralquellen von Wildungen von F. Dreves, und August Wiggers. Gottingen,* 1835.

These wells are situated near hills of greywacke, greenstone, limestone and sandstone. They contain much iron held in solution by carbonic acid; the carbonic acid was estimated by ammonia and chloride of barium; and the barytes being thrown down by sulphuric acid, indicated the quantity of sulphate of barytes corresponding to a proportional quantity of carbonate of barytes. This plan is the same as that adopted by H. Rose.

ARTICLE IX.

SCIENTIFIC INTELLIGENCE, &c.

I.—*Proceedings of the Ashmolean Society, of Oxford,* 1835-6.

Dec. 4th,—A COMMUNICATION from the Rev. J. Guillemard, of St. John's College, was read, detailing his observations on the appearance of the Aurora on the evening of the 18th of November.

The following is the substance of his description :—

Soon after eight o'clock, being then about a mile and a half from Oxford, directly north of the city, his attention was arrested by a faint pale ray of light which suddenly shot up from behind a bank of clouds, towards the zenith. This bank of clouds was then resting on the horizon, extending eastward and westward as far as he could see, and rising to about 30 or 35 degrees upwards, occupying the whole of the northern portion of the heavens to this height. A pale silvery light brightened the upper edges, as if the moon were on the point of rising from behind it. The ray of light which had attracted his attention faded away after a few seconds, but was almost immediately succeeded by another, which was much more brilliant, and continued gradually, but steadily to increase, until it reached nearly to the zenith; and at the same time shorter rays shot up, both to the east and west of it, along, and as it were from behind, the edge of the cloud: very shortly, however, they became less and less distinct, and soon were no longer visible. By this time the edge of the cloud was becoming highly luminous, and a light silvery vapour appeared to be gathering along the line which it formed from east to west. A rapid succession of rays of light followed after an interval of a few minutes. He observed that they began to shoot up, first, in the east, and that, after vanishing *there*, appeared in nearly the same form in the west. None of these rays were as yet of any great length. Soon afterwards luminous patches appeared on the cloud, which shifted with great rapidity along the edge, and melting into one another, lighted up the whole mass, particularly at the edge. From some of these luminous patches, rays broke forth, and gave them very much the appearance of a shell when bursting. These lasted but a few seconds, and then a pause occurred. He was then attracted by a very rapid shooting upwards of more brilliant rays than he had as yet observed, and also by the number of smaller rays which appeared parallel with the longer rays. And one peculiarity especially excited his attention. The longer rays were succeeded by a number which at first were much shorter, and as the whole series passed rapidly from east to west, preserving their parallelism, the lower rays lengthened, and the higher sank down, alternately, shifting in this manner with a rapidity which almost baffled the eye. After this singularly beautiful appearance had ceased, there was nothing remarkable in the quarter of the heavens which had been the scene of these various phenomena, except a faint silvery light, which was diffused generally over the whole of the bank of clouds, which still remained in the situation it occupied at first. Soon, however, much longer rays than any which had been hitherto observed, darted silently and rapidly upwards, shooting far into the clear arch of the heavens, and continuing much longer in full brilliancy, and without shifting, than those which had preceded them. These gradually shortened, and faded away in the place where they had first made their appearance. After waiting some time, without seeing any indications of further phenomena, he turned towards Oxford, believing that the Aurora had ceased. Not more than five or six minutes, however, had elapsed, when it appeared as if a strong light

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was shining behind him, and on turning again towards the north, a most beautiful spectacle presented itself : from the zenith down to the bank of clouds before-mentioned, broad curved belts of rays, highly luminous, of a clear silvery brightness, extended from east to west, and dividing the heavens into a half-dome, as it were, which appeared to be supported by these beams of light.   One of these in the west was broader and far more brilliant than the others.   The stars were distinctly visible through them all ; and, after pausing for several minutes, to contemplate the extraordinary beauty of this phenomenon, he was compelled reluctantly to turn his back upon it, and continue his walk to Oxford.   On looking back, however, just before he had reached the Parks, he observed that no change had taken place.   The rays were as clearly defined and as brilliant as at their first appearance, and seemed like the ribs of some vast dome, on the inner surface of which the various constellations had been marked and lighted up.

Feb., 5, 1836.—Professor Powell read a paper on ratio and proportion as treated by Euclid, including an inquiry into the nature of quantity.

The author's object is to vindicate the method pursued in the fifth book of Euclid from the objections of some modern geometers, and to maintain its completeness as referring to *quantity* considered in the most general and abstract point of view.

An anonymous paper was read on "Flamsteed, Newton, and Halley," in reference to the particulars lately disclosed by Mr. Baily's publication of the memoirs of Flamsteed.   The main facts referred to are as follows : --

In 1675, Flamsteed was named Astronomer Royal, with a salary of 100*l.* per annum, though with promises of necessary instruments and assistance.   These promises, however, were not fulfilled.   The only instruments he had were his own, and he paid his assistants. Yet with the utmost spirit and zeal he persevered under all discouragements in making and recording the most valuable series of observations.

Newton was at this time engaged in completing the theory of the moon, and in 1694, urgently requested of Flamsteed observations of the moon's places.   Flamsteed seems to have had no notion of what that theory really amounted to.   However, he gave Newton the observations on express stipulations of secrecy.   He afterwards suspected Newton of breaking the conditions, and in consequence assumed a tone of increasing coldness, and even acrimony.

Meanwhile, he continued his observations on which the catalogue of the stars was to be founded.   His labours and hardships were doubtless great ; but evidently aggravated by the temper of the individual.   Yet Newton recommended the publication of his observations to the patronage of Prince George of Denmark.   This was done by a committee, who were, in Flamsteed's opinion, wholly under the influence of Newton, or rather Halley.   Complaining and grudging, he still allowed them possession of the MSS. of his observations; and in particular the catalogue of the stars, as yet in a very imperfect condition, was given, *sealed up*, into Newton's keeping, as a sort of pledge for its completion.

The first volume was published, and Flamsteed gave up full copies of his observations for the second volume, and brought also a more complete catalogue of the stars, which was deposited in Newton's hands. The whole was stopped by the death of Prince George in 1708.

In 1710, Flamsteed was much annoyed by the appointment of visitors of the Royal Observatory, Newton being at the head. The Queen also undertook to continue the printing, which was re-commenced without Flamsteed's consent, and, as he alleged, in violation of previous contracts. They proceeded to print the observations in a garbled form, and also inserted lunar places from those at first given to Newton under condition of secrecy, together with the catalogue of the stars above-mentioned.

Soon after this, a violent altercation occurred between Flamsteed and Newton, in which (according to Flamsteed) the latter abused him in terms of the most unmeasured virulence, whilst he accused Newton of having broken open and printed the papers delivered to him sealed up.

Flamsteed now broke off all communication with Newton; and proceeded to revise the observations, and to supply deficiencies, with the determination of publishing a perfect copy at his own expense. This he proceeded to do, but finished only the first and a second volume before his death, in 1719. His widow completed that and the third volume, with the aid of Sharp and Crosthwait, his former assistants, in 1725.

The writer, in conclusion, offers some explanation of the case, upon the consideration chiefly of the manifest characters of the parties. He represents Newton as intensely anxious for the publication of the observations: Halley equally so, having other designs in view at the same time : exercising an ascendancy over Newton, and urging him on to practices, which, he no doubt, managed to persuade him were all justifiable; especially under the plea of the royal authority of his commission.

Flamsteed had much just cause of complaint, but grossly exaggerated it. The final outbreak of virulence and mutual abuse partook of the coarseness of the age. The accusations against Newton, of a breach of confidence and honesty in printing the papers intrusted to him confidentially, and even breaking the seals of a deposit, are examined by the writer in detail, and he finds such discordance and, contradiction in the evidences collected from Flamsteed's papers, as to throw the greatest doubt on the accusation.

*Feb.*19.—The President read a paper, entitled, Notes on the Indica of Ctesias. The object of this communication was to investigate the circumstances which had given origin to the many marvellous stories related of India by that writer, and to shew that they had probably been derived from the country to which they referred, being founded partly upon facts, and partly upon erroneous reports, of mythological legends and sculptures. The paper divided the observations of Ctesias into two classes : those concerning the country and its inhabitants, and those concerning its natural history. Under the former, the writer attempted to prove, that the extent, population, and cli-

mate of India, and the character of the people, were, in several respects, accurately described; that the notion of a people of pigmies was in part founded upon an imperfect acquaintance with the aborigines of India, the barbarous tribes still numerous in the forests and mountains of various districts; and, in part, upon mythological beings believed by the Hindoos to exist; and that, in like manner, the other monstrous races of men with dogs' heads and tails, one leg, no heads, were referable partly to the Hindoo Pantheon, and partly to the mountaineers inhabiting the country on the north of the Hindu Koh and Himalaya. Under the second head, or that of Natural History, the writer shewed, that, amidst many fabulous accounts, there were traces of accurate information, and that the cattle, some of the birds, the lac insect, the indigo plant, and the bambu, were evidently intended, by the accounts given of them; whilst, in other instances, as in the martichora and the unicorn, the characteristics of more than one animal had been blended together and embellished with features derived from the monsters of sculpture, Hindu or Persian. In the course of the paper, the etymological explanations of the Indian names which occur in Ctesias, conjectured by former Orientalists, were shewn, for the most part, to be unfounded, and others, drawn from the Sanscrit language, were proposed.

II.—*Observations on the Steam-Engines of Cornwall.* — By W. J. HENWOOD, F. G. S., Lond. and Paris; Hon. M.Y.P.S., Curator of the Royal Geological Society of Cornwall.

To the Editor of the Records of General Science.

SIR,—In the London and Edinburgh Phil. Mag. (VII. 369.) there is a paper by Mr. John Taylor, " on a New Rotative Steam-engine," in which it is stated that we owe the method of working high-pressure steam expansively to Mr. Woolf, and the circumstance of a rotatory single acting engine working expansively, is made to give the title to the communication.

In the same journal (VIII. 20.) I replied, that an engine working on this principle had been erected by Mr. Watt in 1778; and that single-acting rotatory engines working expansively had been in constant use at Binner Downs Mines in this county since 1828; also that Captain Trevithick had worked high-pressure steam expansively in one cylinder before Mr. Woolf had come to reside in Cornwall, or erected an engine in the county.

Mr. Taylor rejoined (VIII. 136.) that " it was no part of his object to discuss whether the engine which he described was new or otherwise;" that it is " much more important to the public to consider the steps by which improvements are worked out to practical advantage, than to indulge in disputes about such originators of an invention as did little more than to broach an idea."

He charges me with love of controversy, and with "losing no opportunity of endeavouring to detract from Mr. Woolf's merits, and

repeats that the use of high-pressure steam expansively was introduced by Mr. Woolf, and not by Capt. Trevithick.

I had prepared a reply to this, but the editor of the Phil. Mag. (Mr. John Taylor's brother) has neither permitted it to appear acknowledged its receipt, nor noticed a second request that I might be allowed a hearing. It had appeared to me desirable to terminate the discussion in the same journal in which it had originated ; but as this is not permitted me, allow me to crave a page in yours for my explanation.

The first attempt to draw general attention to the high duty of *reciprocating* engines in this county was made by me (Edinburgh Journal of Science, IX. 152, and X. 34.), and I have never since resumed the subject, but to correct the inaccuracies of Mr. Taylor and others. In the first of these communications I attempted a discussion of the quantity of water evaporated, of the heat disengaged by the combustion of a bushel of coals, of the utility of the steam-case, of the quantity of heat passing to the chimney, and of the increased elasticity of steam obtaining from a given increase of heat, when out of contact with water.

Now, Mr. Taylor, when charging me with " indulging in disputes about the originators of an idea," &c., should have recollected that he has himself done nothing else on this subject. Moreover, as Dr. Henry justly observes, the *merit* of an improvement is too often its " originator's" only reward.

In the brief historical introduction to the second of these papers of mine, I have shewn that we are indebted to *Captain Trevithick* for our improved cylindrical boilers, and for any advantages derived from the use of steam of great elastic force ; to *Mr. Woolf* for improvements in the workmanship and nice adjustment of the parts of the engines, which had deteriorated after Mr. Watt had left Cornwall ; and to *Captain Grose* for the advantages of covering all the vessels which contain dense steam with thick coatings of substances which transmit heat very slowly, which, however, was contemplated, and to some extent realized, by Mr. Watt.

I repeat, that Captain Trevithick *first* used high-pressure steam expansively in one cylinder ; is Mr. Taylor prepared to deny it ? I also repeat, that Mr. Watt *first* applied a rotatory motion to a single engine working expansively ; will Mr. Taylor " dispute" it ? For, notwithstanding his *last* (Phil. Mag., VIII. 136.) says " it must be evident that it was no part of my object to discuss whether the engine which I described was *new* or otherwise," his *first* paper (Phil. Mag., VII. 369.) is entitled " on a *new* Rotative Steam-engine."

On the origin of the use of high-pressure steam expansively, Mr. Taylor observes (Phil. Mag., VIII. 137.) :—" I have, in another place, recorded Captain Trevithick's engine at Wheal Prosper, and so far done him justice ; but this engine did only about 26 millions duty, *and did not equal other engines then working in the common way.*" On turning to Mr. Taylor's " Records of Mining" (published in 1829), in which, I presume, this act of " justice" was performed, I find, under date 1813, " In the early part of this year, the

best duty was about 26 millions, *by Captain Trevithick, at Wheal Prosper."* Which, respectively, of these two classes of contradictions does Mr. Taylor wish us to believe as his *present* opinions?

I forbear following him in personalities, as they will neither grace the discussion, nor inform the " public."

I have the honour to remain, sir,

Your very faithful humble servant,

W. J. HENWOOD.

1, Morrab Place, Penzance, April 12, 1836.

III.—*Character of Flamsteed, by Mr. Hodgson.*

SINCE there seems, at the present time, a party inclined to " cry up" Newton at the expense of the first Astronomer-Royal, to whom this country is so much indebted, in face of the most powerful evidence, it is pleasing to be able to bring forward proofs of the high character which he held among his unprejudiced contemporaries. Mr. Hodgson, master of the Royal Mathematical School in Christ's Hospital, and a Fellow of the Royal Society, who published an elaborate and able " System of the Mathematics," in two vols., in 1723, has given in that book detailed accounts of the labours of Flamsteed. The author notices the " mural arch of near eight foot radius," which " he (Flamsteed) erected at his own expense," and observes, that " his utmost view was to render every thing that he undertook as easy and ready for use as might be, and whose happy genius always rendered his good endeavours successful." How different from his character given by Newton ! Again, he says of the determination of the position of the fixed stars, " this that great *astronomer,* and most skilful and diligent *observator,* the late Reverend Mr. Flamsteed, appointed by the royal founder himself, has, after thirty years' indefatigable pains, and constant application to the heavens, happily accomplished ; and has left behind him the largest and most exact catalogue that ever the world was enriched with, containing about 3,500 fixed stars, of which 1,000 lie within the Zodiack, to the great honour of the British nation, and the lasting reputation of the author ; a treasure that will render his name valuable to the latest posterity ; and will perpetuate his memory so long as men shall view the heavens, or ships sail upon the ocean." And again, before giving a summary of Flamsteed's works, he observes, as if sensible that he was treading upon delicate ground, but that he had nothing to fear from honest men—" the generous and unprejudiced reader will readily conclude that the frequent mention made of Mr. Flamsteed cannot proceed from any selfish view (since no advantage can arise from praising or flattering the dead), but from a great esteem, grounded upon a just knowledge of the merits of so great a man."— See Hodgson's System, vol. i., pp. 430—513.)

Opinions like these speak volumes, and throw in the shade the reasonings of those who would still attempt to depreciate the ill-used astronomer. Let it be remembered that Mr. Hodgson was a Fellow of the Royal Society, and that the title was then—what it is not now—an indication of scientific acquirements.

IV.—*Discovery of a new Cave, containing Bones.*

THIS cave has been discovered by M. Julie, at Nabrigas, about two leagues from Meyrueis, a small town in Lozere. It is situated in magnesian limestone, about 300 yards above the level of the Junta, a torrent which runs past it. The height is such as to dispense with stooping—the length of the principal gallery is about 300 yards. The surface of the cave is strewed over with large masses of dolomite, whose angles have been broken off, and water saturated with lime, trickles down the sides. The latter produces a quantity of stalagmites and mud. Over, and mixed with the latter, are distributed many bones in an entire state. They are less abundant in the middle of the cave than at its sides. According to M. Joly, these fossils belong to bears and sheep. Those which have been extracted are,

Bears.—A scull of *Ursus Arctoidæus;* three inferior left jaws of adults; one right inferior jaw of an adult; a great number of molar-canine and incisor-teeth, belonging to individuals of all ages; four omoplates, more or less entire; one of them is so very small, that it may have belonged to a fœtus; an adult humerus, the extremities of which are blunted, but not by friction; three adult cubiti; two adult tibiæ; three adult femora; four adult radii; several phalanges: two young crania, one nearly entire; of the other, only the frontal and parietal bones remain, which retain their sutures perfect; five very young inferior jaws, in which the anterior molar is preceded by two, and sometimes by three alveoli; four young humeri with epiphyses; two young cubiti; two young fibulæ.

Sheep.—Two right inferior jaws; a humerus; a cubitus; a calcaneum, and a great number of bones of birds. A fragment of pottery was also found, forming the lower part of a vase. The diameter of its bottom is ·145 metres; that of its sides about a quarter of an inch; their external surfaces being furrowed for the sake of ornament. It has obviously been dried in the fire. No human bones have been found.—(*Bibliotheque Universelle, April,* 1835, p. 356.)

V.—*Remarkable Case of Convulsions.*

The following case is related by Professor Prevost, of Geneva (*Bibliotheque Universelle, April,* 1835), and relates to 14th July, 1803:

The patient was a mild and sensible woman, incapable of feigning complaints to which she was not subject. She had naturally a false voice, but it often happened that, in the midst of a serious conversation, she would begin to sing with correct notes. This attack ended suddenly, and generally in the middle of a tune. If she attempted to renew her singing, the notes were all false. For a long time she could only taste certain kinds of food; if she attempted to convey any other to the mouth, she was seized with a spasm of the gullet, which completely prevented the passage of the food. At one time she could eat nothing but a custard without sugar; at another time

a ragout with oil. About the same time she could eat certain fruits, and not others of the same kind, although it was impossible to distinguish them by the eye, and she could give no reason for her dislike. At this time her sense of touch was so delicate, that she could tell, by merely touching food, whether or not it would agree with her. If she could not relish it, a kind of convulsion affected her hand and arm, which made her retract it with great rapidity. If she attempted to overcome this repugnance, and convey the food to her mouth, the gullet was constricted, and convulsions commenced by a violent and rapid motion of the head. Presently other movements succeeded, and then the whole body became affected, to as to make her fall from her seat upon the ground. The attack gradually subsided ; but if any one touched her during the paroxysm, the convulsions set in with greater vigour. A remarkable instance of her acute sense of touch was exhibited in the case of a dish of prunes—of the same species—some of which, without her seeing them, produced convulsions, while others had no effect. One of the attendants applied the former to her mouth, when violent convulsions were instantly produced. After being subject to this disease for some years, she had two attacks of fever, and recovered. However, vanilla always produced a slight spasm, and a kind of fainting. Bark was given to her during her illness. Her physician found that she disliked some kinds of bark and not others, always detecting the difference by the hand.

VI.—*Botanical Society of Edinburgh.*

WE rejoice to observe that a Botanical Society has been established in Edinburgh. At a meeting which was held an the 17th of March, the Society was constituted under the title of the " Botanical Society of Edinburgh," the meetings to be held on the second Thursday of every month, from November to July inclusive.

Professor Graham has been elected President, and Drs. Greville and Balfour Vice-presidents of the Society for the present year.

The advancement of Botanical Science is the object of the Society. Its operations will for some time be confined principally to the holding of periodical meetings, to correspondence, to the formation of an herbarium, and the interchange of specimens. The last is a new feature in the constitution of such a society, and will be conducted by a committee, in accordance with certain rules embodied in the laws. The desiderata of botanists in all parts of the kingdom will be supplied, as far as possible, from the Society's duplicates ; and individuals will secure the important advantage of exchanging the botanical productions of their respective districts for those of others more remotely situated. The benefits resulting to science, as well as individuals, by this arrangement, will, it is hoped, be considerable ; especially in regard to the geographical distribution of plants in the British islands, and in the formation of local floras. The society, besides, contemplates an extension of this plan by promoting an exchange of specimens with botanists in other parts of the world.

The Members will be divided into the following classes: Resident, Non-Resident, Foreign and Associate. Any person wishing to become a Non-Resident Member must be recommended by two individuals belonging to some scientific or literary society, and pay a contribution of two guineas, which, without any additional payment, will entitle him, as long as he continues annually to send specimens to the Society, to a participation in the duplicates. To become a Foreign Member, it is necessary to transmit 500 specimens, including at least 100 species, or a botanical work of which the candidate is himself the author ; the former alternative only entitling him to a share of the Society's duplicates. To continue to participate in these duplicates, he must afterwards contribute annually 300 specimens, including at least 50 species.

The Flora of Edinburgh, which is particularly rich, will afford a constant supply of valuable duplicates, and others will be regularly obtained from other parts of Scotland, especially the rarer alpine species.

Local Secretaries will be appointed in different parts of the kingdom. In the meantime, all communications are to be addressed (postage paid) to the Secretary, W. H. Campbell, Esq., 21 Society, Brown-square, Edinburgh.

VII.— *On the Instinct of the Water-Hen.* By P. J. SELBY, Esq.*

As the following anecdote seems to indicate a degree of intellect, or an exercise of the reasoning power, in the feathered race, and apparently acting in conjunction with *Instinct*, or that blind impulse to perform certain offices or actions, for which the lower orders are remarkable, and which, according to the views of one of our ablest naturalists, is supposed, and with great probability, to be the result of physical action upon organizations adapted to receive and respond to it, I think it may not be altogether uninteresting to the Club, and may possibly direct the attention of some of its members more immediately to the various phenomena exhibited by the lower animals in regard to their instincts, combined, or acting as it were, at times in conjunction with the exercise of their reasoning or intellectual faculties. During the early part of the past summer, a pair of water-hens (*Gallinula chloropus*) built their nest by the margin of the ornamental pond at Bell's-Hill, a piece of water of considerable extent, and ordinarily fed by a spring from the height above, but into which the contents of another large pond can occasionally be admitted. This was done while the female was sitting, and as the nest had been built when the water-level stood low, the sudden influx of this large body of water from the second pond caused a rise of several inches, so as to threaten the speedy immersion and consequent destruction of the eggs. This the birds seem to have been aware of, and immediately took precautions against so imminent a danger. For when the gardener (upon whose veracity I can safely

* Proceedings of the Berwickshire Naturalists' Club, p. 84.

rely,) seeing the sudden rise of the water, went to look after the nest, expecting to find it covered, and the eggs destroyed, or at least forsaken by the hen, he observed, while at a distance, both birds busily engaged about the brink where the nest was placed, and, when near enough, he clearly perceived that they were adding, with all possible dispatch, fresh materials, to raise the fabric beyond the level of the increased contents of the pond, and that the *eggs* had, by some means, been removed from the nest by the birds, and were then deposited upon the grass, about a foot or more from the margin of the water. He watched them for some time, and saw the nest rapidly increase in height, but, I regret to add, that he did not remain long enough (fearing he might create alarm) to witness the interesting act of the re-placing of the eggs, which must have been effected shortly afterwards; for, upon his return, in less than an hour, he found the hen quietly sitting upon them in the newly-raised nest. In a few days afterwards, the young were hatched, and, as usual, soon quitted the nest, and took to the water with their parents. The nest was shewn to me *in situ* very soon afterwards, and I could then plainly discern the formation of the new with the older part of the fabric.

VIII.—*Corrections of the Paper on Spirits published in Records*, vol. i. pp. 122 and 125.

To the Editor of the Records of General Science.

Sir,—The science and precision of the theory in the paper on Spirits in your 6th Article for March 1835, induced me to investigate it with more than common attention, and it was with regret that I discovered in the numerical details some inaccuracies, which, to a certain degree disfigure the beauty of the other parts of the article. I allude to the table in page 228, in which, in four instances, the *condensation* added to the *mean* specific gravity does not agree (as it does in the rest of the table) with the specific gravity of the *mixture*. In the seond line, the specific gravity of the mixture, (as there stated) is 0·00231greater than the condensation added to the specific gravity of the *mixture*.

In the 8th line, it is, 0·00009 greater, &c.
In the 9th ditto, it is, 0·00001 less, &c.
In the 11th ditto, it is, 0·00020 less, &c.

In calculating by the well-known formula $\dfrac{(W + w)\,Pp}{Pw + pW} = M$ the mean specific gravity of the alcohol and the water, I have gone to the 6th decimal, which is one more than in the table, and, by a comparison, you will perceive, that, even supposing one-third to be added to the 5th (the last decimal in this table) whenever the 6th is greater than 5, there are many quantities, which differ, and one or two considerably.

Mean Sp. Gr.

0.80944	6
0.81388	9
0.82223	6
0.84331	7
0.87335	5
0.89372	8
0.90845	5
0.91959	7
0.92832	1
0.93534	8
0.94110	2
0.94592	4
0.85001	5

In the continuation of the paper, Article III., for April, page 257, the seventh and eighth lines require the following correction in the pointing:—" for each degree the spirit under trial is above or below 60°."

I am, sir, yours, &c.,

London, 3rd Feb., 1836. OBSERVER.

IX.—*Cases of Poisoning.*

1. *By Acetate of Morphin.*—Some authors have asserted that this salt is not poisonous. The following communication from Pont Mousson, Meurthe, exposes this fallacy:—M. R., judge of the civil tribunal at Sedan, came to an inn of this town. He had arrived from Metz, where he had lost a trifling cause, and was on his way to Plombieres, to drink the waters. After having supped, he went to bed. Next morning he asked for a cup of tea, which was given him. Some time afterwards he came down stairs, and ordered a physician to be called. The unfortunate man had mixed a quantity of acetate of morphin with the tea, and had imagined that its effects would have been instantaneous. Disappointed, he now thought that instead of death he would be subjected to agonizing suffering, and requested Dr. Remelot to administer an antidote. Unfortunately, all efforts were fruitless. M. R. died in the course of the day.

2. *Agaricus Myomica* (lamellis luteis).—A woman, aged sixty years, after eating three agarici of this kind, which had been dressed with salt and oil, was seized in half-an-hour with nausea and vomiting of colourless mucous and filamentary matter. In a quarter of an hour the lower extremities became so weak, as to be unable to support the weight of the body. She lay down, and after taking some cinder-ley, and three or four ounces of oil, Dr. Ghiglini was called in. He administered seven or eight ounces of olive-oil, a clyster with the infusion of chamomile flowers, and a draught, consisting of two ounces of fennel-water, twenty-four grains of Hoffman's liquor, and twenty drops of laudanum. Vomiting recurred. The upper and lower extremities were very cold; pulse and respiration almost insensible; convulsions of the jaws; moaning; violent headache, and pain in the lumbar regions. The same potion

was administered, the extremities were rubbed with hot flannel, and the abdomen fomented with infusion of chamomile. After some relapses, the symptoms abated. Next day headache was present, the pulse and the extremities were very weak. In three days digestion was restored, but the debility was great. This agaricus must be classed along with *narcotico-acrid* poisons, as *agaricus piperatus, acris, &c.—(Journ. de Chim. Medic.*, i. 490.)

3. *Colchicum Autumnale.*—Dr. Schneider relates the cases of two cows, which died in consequence of eating some meadow-grass, in which were the seeds of the autumnal crocus. Opium, decoction of linseed and chamomile-flowers were given without producing any effect.—*(Henke's Zeitschrift fur Staatsarzneikunde*, xxviii. 283.)

X—*New Minerals.*

1. *Sulphuret of Nickel and Bismuth.*—This mineral is found in the district of Sayn Altenkirch, occurring along with quartz and copper pyrites. It crystallizes in octahedrons. Lustre metallic. Colour, light steel gray. Hardness, between that of fluor spar and apatite. Before the blow-pipe, upon charcoal, it gives out in the oxydating flame the odour of sulphurous acid, and after being long exposed to the blast, leaves a metallic-grain which is attracted by the magnet. It affords no fumes of arsenic or antimony. With soda, a sulphuret is obtained and a white metallic grain which is magnetic. With borax, in the oxydating flame, a transparent brown glass is formed; in the reducing flame, a glass possessing a similar colour but mixed with precipitated nickel. With salt of phosphorus a brown glass is formed, which on cooling, becomes faintly green. The specific gravity could not be determined, in consequence of the quartz with which it was mixed. It dissolves readily in acids. Its constituents, according to Kobell are, sulphur 38·46; nickel 40·65; iron 3·48; cobalt 0·28; bismuth 14·11; copper 1·68; lead 1·58. Its composition may be represented by 8 Nk Sl + Bs Sl⁴.—*(Journal fur praktische Chemie*, vi. 332.)

2. *Oerstedite.*—This mineral described by Forchhammer, occurs at Avendal, commonly seated in augite crystals. Colour, brown-splendent. Crystals belonging to the compound pyramidal system. The terminal angle of the first pyramid is 123° 16' 30". The shape has some resemblance to that of Zircon, the angle of which is 123° 19'. Specific gravity 3·629. Hardness, between felspar and apatite. It consists of silica 19·708; lime 2·612; magnesia 2·047; protoxide of iron 1·136; titanic acid and zirconia 68·965; water 5·532; manganese a trace.—*(Poggendorff's Ann.* xxxv. 630.)

3. *Bin-arseniet of Nickel.*—Mr. Booth has analyzed this mineral, from Riechelsdorff, in Hess. Its colour is tin white with a tinge of blueish gray. Fuses before the blow-pipe into a metallic bead, giving out arsenic and into a blue glass with borax. Its constituents are, nickel 20·74; cobalt 3·37; iron 3·25; arsenic 72·64. The nickel and cobalt were separated according to the method of Laugier, that of Phillips having failed after repeated trials.—*(Silliman's Journal*, xxix, 241.)

HORARY OBSERVATIONS OF THE BAROMETER, THERMOMETER, &c.

(Made at the Manse of the Parish of Abbey St. Bathan's, Berwickshire, Lat. 55o 52′ N. Long. 2º 23′ W. at the height of about 450 feet above the sea, for the commencement of each hour per clock, beginning at 6 o'clock in the morning of Monday the 21st March, and ending at 6 o'clock in the evening of Tuesday the 22d, thus extending over 36 hours, according to the suggestion of Sir John Herschel.)   By the Rev. Wallace.

21st Mar. 1835.	Hour.	Thermometer.º	Hygr. (Leslie's)º	Barometer at 320 of Fahrenheit.	Direction of the Wind.	Remarks.
A.M.	6	41	2	29·508	SW by W	Calm, sky cloudless except in S.E. quarter of horizon where there is a bed of cirrostra., with masses of hazy cloud floating below ; in N. E. quarter there is a faint bed of cymoid formation gradually disappearing, also in the N. W. is a patch of cirrostratus.
	7	44	3	29·434	SW by W	Gentle breeze, cirrostra. forming over sky except S. W. On horizon hazy masses floating below cirrostra. About zenith clouds tending to cirrocumulous formation.
	8	45¼	5	29·425	SW by W	Gentle breeze, upper strattum of cirrostra. gradually disappeared, and lower masses of clouds floating copiously over sky.
	9	45¼	3	29·422	SW by W	Gentle breeze, sky completely overcast, fog settling on hills.
	10	46¼	5	29·407	SW by W	Breeze increasing, sky overspread with hazy clouds. Fog has disappeared, but lower atmosphere slightly hazy.
	11	47¾	7	29·390	SW by W	Breeze somewhat brisk sky clearing in zenith northward ; hazy clouds still prevalent southward.
Noon.	12	48¼	10	29·635	SW by W	Brisk wind, sky overspread with hazy clouds in rapid motion.
P.M.	1	49¼	12	29·339	SW by W	Wind rising, sky clearing about zenith, hazy clouds flying from S. W.
	2	49¼	10	29·328	SW by W	Gentle breeze, sky overcast and lowering, with tendency to rain.
	3	46½	6	29·291	SW by W	The same as last hour.
	4	45¾	5	29·285	SW by W	Very gentle breeze, with tendency to fog.
	5	45¼	4	29·257	SW by W	The same as last hour.
	6	45¼	3	29·241	SW by W	Brisk wind, slight drizzle, western qr. very gloomy, sky veiled with cirrostra., soft clouds below driving rapidly before wind.
	7	45	3	29·234	SW by W	Wind strong and gusty ; in other respects same as last hour.
	8	44¼	2	29·229	SW by W	Wind somewhat fallen ; in other respects same as last hour.
	9	44¼	2	29·219	SW by W	The same as last hour.
	10	44¼	2	29·208	SW by W	Wind increased and gusty ; in other respects same as last hour.
	11	44¾	5	29 188	SW by W	The same as last hour.
	12	44¼	4	29·166	S.W by S	The same.
Tuesday. A.M.	1	44¼	4	29·148	S.W by S	Very brisk wind, sky clearing from zenith westward.
	2	43¾	4	29·123	W.	The same as last hour.
	3	43¾	3	29·123	W.	Nearly calm, sky clearing overhead ; heavy clouds round horizon.
	4	43¾	9	29·134	W.N.W.	Wind rising in gusts, western horizon clearing, heavy masses of cloud floating below a higher stratum which nearly veils the sky.
	5	43¾	9	29·140	W.N.W.	Brisk but unsteady wind, sky overspread with cirrostratus, soft clouds floating below.
	6	42½	10	29·162	W.N.W.	Brisk wind, upper stratum of clouds nearly dissipated except on eastern quarter of the heavens ; tendency to formation of nimbus in western quarter.
	7	41½	12	29·184	W.N.W.	Gentle breeze, sky streaked with cirrostratus polarized from N. to S.
	8	42½	17	29·204	W.N.W.	Gentle breeze, sky thinly veiled with cirrostratus.
	9	43½	23	29·216	W.N.W.	Gentle breeze, cirrostra. gradually dissipating and leaving clear sky.
	10	45	29	29·217	W.	Very brisk wind, a thin bed of cirrostratus in the eastern quarter of the heavens, elsewhere, light clouds floating over a blue sky.
	11	47	31	29·225	W.	Very brisk wind, cumulous masses of white cloud floating over a hazy sky.
Noon.	12	46¾	32	29·227	W.	Cirrostratus forming principally in the western quarter ; in other respects the same as last hour.
P.M.	1	47	36	29·235	W.N.W.	Very brisk wind, eastern quarter of the heavens thinly veiled with cirrostratus, everywhere else cumulous masses of cloud floating on a blue sky.
	2	46¼	32	29·230	W.N.W.	The same as last hour.
	3	47½	39	29·239	W.N.W.	The same.
	4	45¼	37	29·243	W.N.W.	Very brisk wind, atmosphere hazy, a few cumulous masses of cloud in the west.
	5	43¾	26	29·260	W.N.W.	Cloudless, wind falling.
	6	41	21	29·269	W.N.W.	The same as last hour.

Meteorological Journal,

...pt at the Manse of the Parish of Abbey St. Bathan's, Berwickshire, Lat. 55° 52′ N. Long. 2° 23′ W. at the height of about 450 feet above the sea. By the Rev. JOHN WALLACE.

MARCH.

DATE 1856	THERMOMETER IX A.M.	X A.M.	III P.M.	XI P.M.	HYGROMETER (Leslie's) IX AM	X AM	III PM	XI PM	BAROMETER At 32° of Fahrenheit IX A.M.	X A.M.	III P.M.	X. P.M.	Rain in inches Weekly	Direction of Wind at X.A.M.	REMARKS
Tu. 1	34½	33½	36	35½	1	0	2	0	28·584	28·558	28·488	28·330		E. S. E.	Strong wind with sleet and snow, rain in the evening.
We. 2	36¼	37¼	38½	33	5	8	14	11	28·469	28·483	28·515	28·850		S. W.	Brisk wind, sky thinly veiled with cirrostratus, evening calm.
Full M. Th. 3	36	36⅜	40	35¼	6	8	10	7	28·994	29·018	29·055	29·051		S. W.	Gentle wind, cirri and cirrostra. prevalent, light clouds floating, evg. hazy.
Fr. 4	38¼	39¼	41¼	38½	5	19	14	7	29·020	29·013	28·577	28·577	1·625	S.	Gentle wind, A.M. cirri, soft clouds floating, P.M. cirrostra., evg. strong wind.
Sa. 5	38¼	37¾	40¼	38¼	15	19	21	3	28·652	28·685	28·672	28·604		SW.by W.	A.M. gentle wind, fine and nearly cloudless, P.M. snow-showers, evg. calm.
Su. 6	34	36	37¼	37	10	8	4	1	28·831	28·821	28·639	28·518		E. S. E.	A.M. gentle wind, sky cloudy and lowering, P.M. windy with rain, evg. clear.
Mo. 7	37	39	43	34¼	10	13	15	4	28·722	28·746	28·763	28·819		S. by E.	Brisk wind, A.M. showery, M.P. masses of cloud prevalent, evg. showery.
Tu. 8	36	37¼	39¼	29¼	5	7	9	5	28·899	28·914	28·939	28·975		N. by W.	Brisk wind, A.M. clear, noon gloomy, P.M. nearly cloudless, evg. clear, deposition.
We. 9	33½	35½	39¼	29¼	4	20	13	0	28·851	28·864	28·793	28·893		S. S. E.	Calm and nearly cloudless, evening clear with deposition.
Th. 10	35½	37	40¼	36	17	15	17	0	28·877	28·831	28·699	28·451		S. E.	Calm, cirrostratus and cirrocumulus prevalent, P.M. overcast, evening rain.
Fr. 11	36	36½	39½	36½	3	3	11	1	28·333	28·318	28·314	28·297	0·625	S. E.	Gentle wind, A.M. slight fall of snow, P.M. cloudy, rain in the evening.
Sa. 12	40	44½	43	36½	5	3	11	6	28·279	28·301	28·417	28·635		N. N. W.	Brisk wind, overcast with cirrostra, occasional showers.
Su. 13	37¼	38¼	42½	38½	12	15	27	2	28·884	28·905	28·881	28·561		SW.byW.	Very brisk wind, cirrostra. prevalent, light clouds floating, evg. wind and rain.
Mo. 14	38¼	39⅜	39¼	35	10	12	14	11	28·192	28·188	28·240	28·462		NWbyW.	Strong wind, sky nearly cloudless, occasionally passing showers, evening clear.
Tu. 15	38	38½	41	36	14	14	22	4	28·472	28·500	29·106	28·696		W. by N.	Brisk wind, sky thinly veiled with cirri and cirrostratus, evening cloudy.
We. 16	35	35	39½	39¼	5	5	16	4	28·917	28·929	29·106	29·220		W.	Gentle wind, frequent snow and hail-showers, evening calm and cloudy.
New M Th. 17	44½	48½	45	39	4	19	21	11	28·716	28·705	28·847	29·149		NWbyW.	Boisterous and gusty wind with troubled sky and frequent heavy showers.
Fr. 18	42½	45	48½	48	16	22	12	11	29·557	29·557	29·568	29·550	0·615	W. S. W.	Brisk wind, cirrostratus (cyrnoid formation) prevalent, evening clear.
Sa. 19	46½	48½	54½	45	10	11	36	7	29·550	29·539	29·491	29·522		W. S. W.	Gentle wind, fine and nearly cloudless, evening calm and cloudy.
Su. 20	46½	51¾	56½	42	13	20	53	6	29·583	29·577	29·544	29·556		S. W.	Calm, fine with a cloudless sky.
Mo. 21	45½	46¼	44½	44½	3	5	6	8	29·422	29·407	29·291	29·208		SW.byW.	Brisk wind, cloudy and lowering, occasionally driving rain.
Tu. 22	43½	45	47½	36	23	29	39	8	29·216	29·217	29·230	29·275		SW.byW.	Brisk wind, cirrostra. prevalent with cumuli floating below, evening calm.
We. 23	42	43	41½	39¼	12	10	7	4	29·018	28·971	28·737	28·626		S. S. E.	Gentle wind, cirrostratus abundant, occasional showers, evening calm.
Th. 24	40¼	40⅜	38	31¼	19	22	15	7	28·614	28·644	28·606	28·602		W. S. W.	Calm, cirrostratus and floating cumuli prevalent, snow-showers, evg. calm.
Fr. 25	38¼	40	41¼	35	14	20	23	7	28·353	28·342	28·269	28·256	0·798	W. S. W.	Brisk wind, cirrostra. with cumulative clouds below, evening calm, deposition.
Sa. 26	40½	42	42½	33	17	16	17	12	28·374	28·408	28·523	28·748		W. N. W.	Very brisk wind, A.M. cumulative masses of cloud, P.M. occasional showers.
Su. 27	39	40½	40¼	31	18	20	20	2	28·836	28·817	28·679	28·693		W.N.W.	Strong wind, P.M. overcast, sky troubled, snow-showers, evg. calm and clear.
Mo. 28	34½	36¼	36½	33	12	7	4	1	28·623	28·613	28·576	28·585		S.E.by S.	Gentle breeze, sleet and snow showers, breeze encreases in evening.
Tu. 29	37	37¾	40	31¼	10	11	14	16	28·706	28·745	28·771	28·843	0·015	W. by N.	Very brisk wind with abundant cirri and cirrostra... evening.
We. 30	40	40¼	40	36½	15	23	3	1	28·921	28·904	28·722	28·641		S.E.byW.	Gentle breeze, A.M. cloudy, P.M. drizzling rain
Th. 31	40	41	40½	36½	14	15	17	10	28·653	28·708	28·807	28·831		N. N. W.	Boisterous wind with snow-showers, sky unsettl... y cloudless.

Means { Therm. 39·3 } Mean Barom.

RECORDS

OF

GENERAL SCIENCE.

ARTICLE I.

Memoir of Dr. Thomas Young.   By M. ARAGO.

(*Concluded from* page 332.)

DISCUSSIONS with regard to priority of discovery, even
when influenced by national prejudices, would never become
bitter, if they could be resolved by fixed rules, but, in cer-
tain cases, the first idea is every thing; in others, the de-
tails present the principal difficulties; while in other re-
spects, the merit appears to depend less on the conception
of a theory, than on its demonstration.

To remove the difficulties which surround the settlement
of such a question, I have sought for an example in which
the claims of the two pretenders to the invention may be
likened to those of Champollion and Young, and which
has been decisively settled.   This example, I consider,
exists *in the interferences*, laying entirely aside for the hiero-
glyphical question, the references appended to the memoir
of M. de Guignes.

Hooke, in fact, had stated before Young, that the rays
of light interfere, just as the last had supposed, before
Champollion, that the Egyptian hieroglyphics are some-
times phonetic.   Hooke did not prove his hypothesis di-
rectly; the proof of the phonetic values assigned by Young
to different hieroglyphics, could only be deduced from
readings which have not been made—which have never
existed.

From want of knowledge of the composition of white light, Hooke had not an exact notion of the nature of interferences, as Young was deceived by an assumed syllabic or dissyllabic value of the hieroglyphics.

Young is universally allowed to be the discoverer of the theory of interferences; hence, by a consequence which appears to me inevitable, Champollion should be regarded as the discoverer of the hieroglyphics.

Had Young, while alive, been allowed to choose the alternative of being considered the author of the doctrine of interferences, leaving the hieroglyphics to Champollion, or of retaining the hieroglyphics and abandoning to Hooke the ingenious optical theory, I have no doubt that he would have readily recognized the claims of our illustrious countryman.

Farther, no one can dispute with him the title of being distinguished in the history of the memorable discovery of the hieroglyphics, as Kepler, Borelli, Hooke, and Wren, figure in the history of universal gravitation.

The limits of this Memoir do not permit me even to cite the simple titles of the numerous papers which Dr. Young published: yet the publication of such a rich catalogue would have certainly been sufficient for his honour. Who would not suppose that we were enumerating the labours of several academies, and not those of a single person, as in the following series?—

Memoir on Iron Furnaces; Essays on Music and Painting; Researches into the Habits of Spiders, and the System of Fabricius; On the Stability of the Arches of Bridges; On the Atmosphere of the Moon; Mathematical Theory of Epicycloidal Curves; Restitution and Translation of different Greek Inscriptions; On the Means of strengthening the Timber-work of Wooden Vessels; On the Action of the Heart and Arteries in the Phenomenon of Circulation; Theory of the Tides; On the Diseases of the Chest; On the Friction in the Axes of Machines; On the Yellow Fever; On the Calculation of Eclipses; Essays on Grammar, &c.

Such a variety of elaborate works one would have thought sufficient to shut up their author in his study. Thomas Young, on the contrary, was a man of the world.

He frequented, assiduously, the most brilliant circles in London. The accomplishments of his mind, the elegance of his manners, were fully sufficient to render him a remarkable person; but when we consider these numerous combinations, in which 50 different subjects are developed in a few minutes, we can then conceive the value of such a true living library, where an exact, precise, and substantial answer could be given in an instant to every kind of question which could be proposed.

Young devoted much time to the study of the arts. Several of his memoirs display the profound knowledge which he acquired at an early period of the theory of music. I shall not dwell on his powers of execution, because there were two instruments on which he could not perform, and I do not know what they were. His taste for painting was acquired during his stay in Germany. There his attention was entirely taken up with the collection at Dresden; for he did not aspire at acquiring a mere superficial knowledge of them, but he studied the defects and the peculiarities of the greatest masters, their frequent changes of style, the material objects of their work, and the modifications which the objects and colours underwent in the lapse of time. Young, in short, studied painting in Saxony in the same manner as he had previously studied the languages in his own country; and, as latterly, he cultivated the sciences. In fact, every thing appeared, in the sight of Young, worthy of meditation and research. The college-acquaintances of the illustrious philosopher recollect an amusing example of this turn of mind. They tell that, having entered the room of Young during the day that he had received at Edinburgh the first lesson in dancing a minuet, they found him busy, with a rule and compass, measuring the intersecting directions which the two dancers followed, and the different improvements which the various figures appeared to him susceptible of acquiring.

Young acquired at an early period from the sect of the Quakers, to which he belonged, the opinion that the intellectual faculties of children differ originally much less from each other than is commonly supposed. *Every man ought to be able to do what every other man has done*, had become his favourite maxim. He never retracted from submitting

2 D 2

even his own person to prove the correctness of his system. The first time that he mounted a horse, in company with the grandson of Mr. Barclay, the riding-master who accompanied them leaped over an elevated barrier. Young wished to follow him, but was driven over the horse's head ten feet. He got up without a word—made a second trial —was again dismounted—but did not pass this time the head of the animal. On the third trial the young student succeeded in accomplishing what had been done before him. This practice would not have been mentioned, had it not been engaged in, first at Edinburgh, then at Gottingen, and carried much farther than one would be apt to believe.

In one of these two cities he vied successfully with a distinguished rope-dancer; in the other, and always in consequence of a challenge, he acquired an extraordinary facility in the art of vaulting on a horse; and was remarked, even among skilful equestrians, whose striking feats attracted nightly crowds to the circle of Franconi. Thus, those who are pleased with contrasts, might represent on the one side Newton—the timid Newton—riding in a carriage, with his arms extended and grasping the coach doors, from the fear of falling with which he was impressed; and, on the other hand, his illustrious competitor galloping erect on two horses, with all the assurance of a professional riding-master.

In England, a physician, if he wishes to retain the confidence of the public, should abstain from every kind of scientific or literary work which is foreign to the practice of medicine. Young gave up to this prejudice for a long time; his papers appeared anonymously. This veil, however, was very transparent; two contiguous letters of a Latin motto, in a regular order, formed the signature to each memoir, but Young communicated the three Latin words to all his friends, both national and foreign, without advising them to keep the information secret. But further: who could be ignorant that the illustrious author of the Theory of Interferences was the foreign secretary of the Royal Society of London; that he gave, in the theatre of the Royal Institution, a general Course of Mathematical Physics; that, in connexion with Sir Humphry Davy, he

published a scientific journal, &c., &c.?   On important oc-
casions, as, for example, when his two quarto volumes,
each consisting of 800 or 900 pages, appeared in 1807,
where every branch of natural philosophy was treated in a
new and profound manner, he forgot the interest of the
physician, and the name of Young, in large letters, re-
placed the two small italics, whose turn had now arrived,
and which would have made such a ridiculous figure in this
stupendous work.

Young never had, either when at London or Worthing,
where he passed the sea-bathing season, a very extensive
practice.   The 'public found him too learned!   It is even
true that his lectures on the Practice of Medicine, at St.
George's Hospital, were ill attended.   It has been said, in
explanation of this, that his lectures were too substantial—
that they were above vulgar intellects!   Might not this
want of success be rather attributed to the frequent oppor-
tunities on which Young pointed out the inextricable diffi-
culties which are met with at each step in studying the
numerous disorders of our frail machine?

Could a professor of the faculty at Paris retain any audi-
dience, at this time especially, when every one is anxious
to obtain his object rapidly, and without labour, if he made
use of such language as that which I now transcribe from
Young?

" No study is so complicated as that of medicine; it ex-
ceeds the bounds of human intelligence.   Physicians who
act precipitately, without attempting to comprehend what
they observe, are often as much in error as those who
constantly generalize from observations which are not
analogical."

And if the professor, continuing in the same strain,
added—" In the *lottery* of medicine, the chances of the
possessor of ten tickets are evidently greater than those
of the person who has only five."

When they believed themselves engaged in a lottery,
would those of the audience, whom the first expression had
not dissipated, be disposed to make great efforts to obtain
the greater number of tickets, or, explaining the idea of
Young, as much knowledge as possible?

Notwithstanding his knowledge, perhaps on account of

its extent, Young was quite destitute of confidence at the sick bed. Then the vexatious effects which might eventually result from the action of the best medicine recommended being presented at once to his mind, appeared to him to balance the favourable chances which should be expected, and made him undecided; doubtless a natural feeling, but one which people never relish. The same timidity was displayed in all the medical works of Young. This man, so remarkably distinguished for the boldness of his scientific views, could only give on this science simple catalogues of facts. He was scarcely convinced with the goodness of his thesis, either when he attacked the celebrated Dr. Radcliffe, whose whole secret, in a most brilliant and successful practice, as he himself declared, was to employ contrary remedies, or when he contended with Dr. Brown, who found himself, said he, under the disagreeable necessity of acknowledging, from the evidence of the official documents of an hospital under the care of justly celebrated physicians, that the majority of fevers, when left to themselves, are neither more severe, nor of longer duration, than when they are treated by the best methods.

In 1818, Young having been appointed Secretary to the Board of Longitude, gave up almost entirely the practice of medicine, in order to devote the most careful attention to the celebrated periodical work called the *Nautical Almanac*. After this period, the journal of the Royal Institution contained, quarterly, numerous dissertations upon the most important problems of navigation and astronomy. A volume, entitled *Illustrations of the Mecanique Celeste of Laplace;* a philosophical dissertation on the *Tides*, fully attested that Young did not consider his situation a sinecure. In this employment, however, there was presented to him one inexhaustible source of disgust. The *Nautical Almanac* had hitherto been a work exclusively devoted to the sea service. Some individuals requested that it should constitute a complete astronomical ephemeris. The Board of Longitude not having appeared to decide in favour of the projected change, was suddenly exposed to the most violent attacks. The journals of every description, *Whig* or *Tory*, took part in the discussion. Nothing was seen in

the union of Davy, Wollaston, Young, Herschel, Kater, and Pond, but a number of individuals who were subservient to a Bœotian influence. The *Nautical Almanac,* already so celebrated, had become an object of disgrace to the English nation. If a typographical error was discovered in it, as there always will be in a large volume of cyphers, the British navy, from the smallest sloop to the largest decker, was about to be swallowed up in the depths of the ocean.

It has been asserted that the principal promoter of these foolish exaggerations could not detect any considerable errors in the *Nautical Almanac,* after having fruitlessly endeavoured to collect them for the Board of Longitude. I do not know if the fact is correct. In every case it is improper to re-echo the reports of malice. I cannot forget that for several years the member of the Royal Society, who has been mentioned, nobly devoted a large portion of his princely fortune to the advancement of science.

This plausible astronomer, like all philosophers whose minds are concentrated upon one object, unfortunately and inexcusably appeared to exaggerate subjects to which he had directed his attention; but he ought to be found fault with for not recollecting that his exaggerations would be taken seriously by many. He forgot that in all countries, and at all times, there exists a great number of individuals who, distressed at their own obscurity, seize as their prey every opportunity of scandal; and, under the mask of the public good, criticise, without mercy, those of their cotemporaries whose celebrity proclaims their success. At Rome, he who was charged with insulting a victorious general was a slave; at London, distinguished philosophers were cruelly attacked by a member of the House of Commons.

The abolition of the Board of Longitude was speedily the consequence. The health of Young, which had been delicate, declined after this sad period with great rapidity. The able physicians who attended him speedily lost all hope of him. He was conscious of his approaching end, and viewed it with admirable calmness. Till his last hour he was occupied, without ceasing, with his Egyptian Dictionary, then in the press, and which was not published till after his death. When his strength would not allow

him to rise and use a pen, he corrected the proofs with a pencil. One of the last acts of his life was to request the suppression of a talented paper, written by a friend, against all those who had contributed to the destruction of the Board of Longitude.

Young died in the midst of a family by whom he was beloved, on the 10th of May, 1829, about the age of 56 years. The inspection of his body after death shewed that his aorta was ossified.

Young is, in the eyes of the men of science of France, one of the most illustrious philosophers of whom England can boast. The previous details would lead us to infer that just honours have been paid to the author of such a fine discovery as the Law of Interferences. These antici- pations, I am sorry to say, have not been realized. The death of Young made but little stir in his own country. The doors of Westminster, hitherto so accessible to per- sons destitute of merit, but with titles, have remained closed to the man of genius, who was not a baronet. It is at the village of Farnborough, in the modest vault of his wife's family, that the remains of Thomas Young have been deposited. The indifference of the English nation to labours which add so much to its honour, is a very ano- malous circumstance, of which one is naturally curious to know the cause.

I should be deficient in candour—I should be a pane- gyrist instead of a historian—if I did not admit that Young did not in general carry with him the intelligence of his readers, and that most of his writings are distinguished by a certain degree of obscurity.

The exact sciences possess an advantage over works of art or imagination, which has been often pointed out. The truths of which they consist pass through ages, without suffering from the caprice of fashion, or the depravity of taste. But when we attain a certain height, upon how many judges can we depend? When Richelieu let loose upon the great Corneille a number of those individuals whom the merit of another renders furious, the inhabitants of Paris hissed the adherents of the bishop, and applauded the poet. This homage is denied to the geometrician, the astronomer, and the natural philosopher, who cultivate the

highest branches of science. Those who are competent to
form an opinion on their labours, never exceed eight or ten
in all Europe. Suppose them to be unjust, indifferent,
nay, even jealous—for such is often the case—the public,
obliged to depend upon their information, would be igno-
rant that Alembert had traced the great phenomenon 'of
the precession of the equinoxes to the principle of univer-
sal gravitation; that Lagrange determined the physical
cause of the libration of the moon; that from the researches
of Laplace the acceleration of the motion of this star is
found to be connected with a particular change in the
form of the earth's orbit, &c. Scientific journals, when
they are conducted by men of distinguished merit, acquire,
in this way, an influence on certain subjects, which is often
distressing. Such is the power which the *Edinburgh Re-
view* has sometimes exercised. Among the first contribu-
tors to this celebrated journal, a young writer was highly
distinguished, in whom the discoveries of Newton had
produced a powerful admiration. This feeling, so natural
and so legitimate, made him unfortunately misconceive all
that was plausible, ingenious, and fertile, in the theory of
interferences. The author of this theory did, perhaps, not
always write his decisions, his opinions, and criticisms, in
such polished language as is proper on common occasions,
and as was most imperiously required, when proceeding
from the immortal author of the *Natural Philosophy*. The
penalty of retaliation was applied to him with interest.
The *Edinburgh Review* attacked the learned man, the
writer, the geometrician, the experimenter, with a vehe-
mence and acrimony of expression almost unexampled in
scientific debates. The public are commonly on their
guard when they are addressed in such passionate lan-
guage; but, on this occasion, it adopted the opinions of the
reviewer, without accusing him of levity. The reviewer
was not one of those youthful critics unqualified, by want
of previous study, from undertaking the task which he had
assigned to himself. Several good memoirs in the collec-
tion of the Royal Society attested his mathematical know-
ledge, and assigned him a distinguished place among the
cultivators of physical optics. The bar of London pro-
claimed him already as one of its most shining lights. The

Whigs of the House of Commons beheld in him the caustic
orator, who, in parliamentary debates, was often the suc-
cessful antagonist of Canning.   He was the future Presi-
sident of the House of Peers; he was the present Lord
Chancellor.* But, how could unjust criticism, proceed-
ing from so high a quarter, be opposed?   I know
that certain minds depend implicitly on the consciousness
of their just claims, and in the certainty that, sooner or
later, truth will triumph; but the multitude is inevitably
guided by a sentiment inherent in our nature, which Vol-
taire has thus stated :

> Quand dans la tombe un pauvre homme est inclus
> Qu'importe un bruit, un nom qu'il n'entend plus !

Listen, for example to Galileo himself, after his abjura-
tion, saying in a whisper—

> E pur si muove !

These immortal words were not accidental expressions, but
were the expression of the cruel vexation which the distin-
guished old man experienced.   Young, likewise, in a
pamphlet which he published in answer to the *Edinburgh
Review*, shewed that he was much discouraged.   The live-
liness, the force of his expressions, but ill-disguised the
feeling which oppressed him.   But now let us haste to tell
it : justice—complete justice—has been at last granted to
him.   For some years the whole world beheld in him one
of the principal illustrations of our times.   It was in France
(Young took pleasure in declaring it himself) that the first

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* The periodicals having done me the honour of frequently noticing the nume-
rous testimonials of kindness and friendship with which Lord Brougham favoured
me in 1834, both in Scotland and at Paris, a few words in explanation appear ne-
cessary.   This Memoir of Dr. Young was read at a public meeting of the Aca-
demy of Sciences, on the 26th of November, 1832.   At this period I had no
personal knowledge of the author of the articles in the *Edinburgh Review*, I there-
fore cannot be accused of ingratitude.   But it might be said, Why did you not
suppress entirely the history of this matter when you were about to publish?   I
might have done so, and the idea had occurred to me; but I soon gave it up.   I
knew too well the elevated sentiments of my illustrious friend to fear that he
would be offended at my candour on a question on which I am thoroughly con-
vinced the immense extent of his mind has not placed under the shelter of error.
The homage which I pay to the noble character of Lord Brougham in publishing
at present this passage in the *eloge* of Young, without modifying it, is, in my opi-
nion, so significant, that I shall not attempt to add more.

signal of this slow reparation was given. I should add, that at an earlier period, when the doctrine of interferences had made no proselytes, either in England or upon the continent, Young found in his own family one who understood it, and whose vote was sufficient to console him for the disdain of the public. The individual whom I shall here point out as entitled to the gratitude of all the philosophers of Europe, will excuse me for finishing my indiscretion.

In the year 1816, I made a tour in England with my scientific friend, M. Gay Lussac. Fresnel had then begun his brilliant and scientific career, by his memoir on diffraction. This work, which in our opinion contained a capital experiment, irreconcilable with the Newtonian theory of light, became the first object of our conversation with Dr. Young. We were astonished at the numerous limitations which he made to our eulogies, when at last he declared that the experiment of which we thought so much had been described since 1807, in his *Treatise on Natural Philosophy*. This assertion did not appear to us well founded. It led to a long and minute discussion. Mrs. Young was present, but appeared to take no part in it. But as we knew that a fear truly puerile of passing for learned women—the fear of being designated by the ridiculous title of *blue stockings*, renders English ladies very reserved in presence of strangers, our want of a knowledge of the world only struck us when Mrs. Young hurriedly left the room. We began to apologise to her husband, when we saw her re-enter, carrying under her arm a huge quarto. It was the first volume of the *Treatise on Natural Philosophy*. She placed it on the table, opened it at page 787, without saying a word, and pointed to a figure where the curvilinear direction of diffracted bands, on which the discussion rested, was theoretically established.

I hope I shall be pardoned for entering into these minute details. Have not too many examples already accustomed the public to consider obscurity, injustice, persecution and misery as the natural reward of those who honourably spend their time in developing the human mind? Let us not forget, then, the exceptions, when they are presented to us. If we wish youth to devote himself with ardour to intellec-

tual labours, let us shew him that great discoveries may produce some peace and happiness; let us cut out from the history of science such leaves as tarnish her lustre; let us endeavour to persuade ourselves that in the dungeons of the Inquisition a friendly voice communicated to Galileo some of those consoling expressions which posterity reserved for his memory; that the thick walls of the Bastille cannot prevent public opinion from apprising Freret that some day the work which he wrote in prison will be one of the titles of his fame; that before dying in the hospital, Borelli found sometimes, in the city of Rome, a shelter from the inclemency of the air, a little straw to recline his head upon; that, lastly, Kepler—the great Kepler— never experienced the pangs of hunger.

ARTICLE II.

Method of determining the Value of Black Oxide of Manganese for manufacturing Purposes. By Thomas Thomson, M.D., F.R.S., L. and E. Regius Professor of Chemistry in the University of Glasgow.

The manganese to be tested must be reduced to a fine powder, or brought into the state in which it is used by the manufacturers of bleaching-powder. To determine its value, proceed in the following manner:

Into a balanced Florence flask put 600 grains of water, and 75 grains of crystals of oxalic acid. Then add 50 grains of the manganese to be tested; and, as quickly as possible, pour into the flask from 150 to 200 grains of concentrated sulphuric acid. This is best done by having a given weight of sulphuric acid, say 210 grains, previously weighed out in a glass measure, counterpoised on one of the scales of a balance. You pour into the flask as much of the sulphuric acid as you can conveniently. Then, putting the measure again into the scale, you determine exactly how much has been put in.

A lively effervescence takes place, and carbonic acid gas is disengaged in abundance. Cover the mouth of the flask with paper, and leave it for 24 hours; then weigh it again.

The loss of weight which the flask has sustained is exactly equal to the quantity of *binoxide* of manganese in the powder examined. Thus, let the loss of weight be 34 grains; the quantity of binoxide of manganese in the 50 grains of the powder which was tested will be 34 grains; or it will contain 68 per cent. of pure binoxide of manganese, and 32 per cent. of impurity.

To understand what takes place, it is necessary to recollect that oxalic acid is composed of

2 atoms carbon	1·5
3 atoms oxygen	3
	4·5

and that binoxide of manganese is composed of

1 atom manganese	3·5
2 atoms oxygen	2
	5·5

The oxalic acid acts on the binoxide by abstracting one-half of its oxygen, which converts it into carbonic acid; hence the effervescence. 55 grains of pure binoxide of manganese would give out 10 grains of oxygen, which would convert 45 grains of oxalic acid into 55 grains of carbonic acid; which escaping, indicate, by the loss of weight, the quantity of carbonic acid formed. Now, it happens that the weight of the carbonic acid formed is exactly equal to the quantity of binoxide of manganese which gives out its oxygen to the oxalic acid. Hence, the reason of the accuracy of the test.

In other words, an integral particle of binoxide of manganese, which weighs 5·5, gives out 1 atom of oxygen. This atom of oxygen combines with an integrant particle of oxalic acid, weighing 4·5, and converts it into two integrant particles of carbonic acid, which both together weigh 5·5. As this carbonic acid escapes, the loss of weight must be just equal to the quantity of binoxide of manganese in the powder subjected to experiment.

In practice, I find that a small quantity of the binoxide of manganese sometimes escapes the action of the oxalic acid, being probably screened by the great quantity of impurity with which it is mixed. But the deficiency of car-

bonic acid occasioned by this is about made up by the mois-
ture which the carbonic acid gas carries off along with it.
This renders the error in general trifling.

It will be proper to subjoin an example or two of the
method of proceeding, to enable the reader to judge of the
goodness of this test, and its value to the manufacturer.

The black oxide of manganese employed was subjected
to analysis, and found composed of

Binoxide of manganese	68·49
Peroxide of iron	11·85
Water	5·68
Earthy matter	13·98
	100·00

Experiment 1.

Put into the flask—Water	599 grs.
Oxalic acid . . .	75
Black oxide . .	50
Sulphuric acid .	184
Total . . .	908

Loss of weight 32·5 grains.　It ought to have been 34·245
grains.　Error 1·745 grains.

Experiment 2.

Put into the flask—Water	600 grs.
Oxalic acid . . .	75
Black oxide . .	50
Sulphuric acid . .	154
Total . . .	879

Loss of weight 34·5 grains. It ought to have been
34·245 grains. Here the error is in excess, and amounts
to 0·255 grains.

Experiment 3.

Put into the flask—Water	600 grs.
Oxalic acid . . .	75
Black oxide . .•	50
Sulphuric acid . .	154·1
Total . . .	879·1

Loss of weight 35 grains. Here also the error was in excess, and amounted to 0·755 grains.

Let us take the mean of these three experiments :

Loss of weight by 1st . . . 32·5 grs.

2nd . . 34·5

3rd . . 35·0

3)102

Mean 34 grains.

Here the error amounts to 0·245 grains, which is considerably less than 1 per cent. If, therefore, three trials be made, the error will be under 1 per cent.; so that the method is quite sufficient to indicate very nearly the quantity of binoxide of manganese in any ore. Now, it is the binoxide of manganese alone that is useful to the manufacturer; the sesqui-oxide and red oxide availing very little in the preparation of chlorine, for which almost alone the ore is used by manufacturers.

I tried various other proportions of the ingredients, but found the preceding the best. I tried, also, the effect of rubbing up in a mortar the oxalic acid and black oxide. But the error is least when the oxalic acid is merely poured into the water, and the black oxide added before the acid is dissolved. Unless the sulphuric acid be added last, we cannot be sure of our weights.

ARTICLE III.

Description and Analysis of Emmonite, a new Species of Carbonated Strontian from America. By THOMAS THOMSON, M. D., F. R. S., L. and E. Professor of Chemistry, Glasgow.

THE only specimen of this mineral which I have seen I got from Mr. Sowerby, who had received it from Massachusetts, under the name of carbonate of strontian, sanctioned by the authority of Professor Emmons, of William's College. It was obvious, from the external characters of the mineral, that it was specifically different from all the varieties of carbonate of strontian that I had seen. I there-

fore subjected it to analysis, and found it a compound of 9
atoms carbonate of strontian, and 2 atoms carbonate of
lime.

The colour of this mineral is snow white ; the structure
obscurely foliated, but imperfect cleavages may be obtained
in the direction of a right oblique prism.   My attempts to
measure the angles of this prism, even with a common go-
niometer, were frustrated by the unevenness of the cleav-
age faces.   I found the great angle of the prism to be 113°,
and the small one 63°.   It is obvious that no reliance can
be placed on these measurements, as the two together, in-
stead of 180°, make only 176°.

Fracture in the direction of the cleavage planes flat and
smooth ; but the mineral in general had a scaly appearance,
not unlike some varieties of gypsum.

Translucent on the edges.

Very easily reduced to powder.

Hardness 2·75.   Specific gravity 2·9463.

100 grains of the mineral were dissolved with efferves-
cence in nitric acid, except a residue of 3·79 grains, con-
sisting of particles of zeolite, quite white, and easily crushed
between the fingers.   The solution, mixed with ammonia,
let fall one grain of peroxide of iron, with a trace of alu-
mina.   The solution, freed by heat from nitrate of ammo-
nia, weighed 139 grains.

I now examined the crystallized matter, and found it to
be a mixture of nitrate of strontian and nitrate of lime, and
nothing else.   The nitrate of lime was separated by digest-
ing the salt in absolute alcohol.   Hence, the composition
was easily deduced.   But it may be determined with equal
accuracy from the following data :—

The carbonates of strontian and lime weighed 95·21 grs.

The nitrates of ditto 139   grs.

The atom of strontian weighs 6·5.   Let the number of
atoms in 100 grains of the mineral be x.

The atom of lime is 3·5.   Let the number of atoms pre-
sent be y.

The atom of nitric acid $= 6·75$.   Number of atoms in
the nitrates $= 6·75 (x + y)$.

The atom of carbonic acid $= 2·75$.   Number of atoms in
the carbonates $= 2·75 (x + y)$.

We have from the nitrates $6\cdot5\,x + 3\cdot5\,y + 6\cdot75\,(x+y) =$ 139; or,

$$13\cdot25\,x + 10\cdot25\,y = 139 \ \& \ x = \frac{139 - 10\cdot25\,y}{13\cdot25}$$

From the carbonate $6\cdot5\,x + 3\cdot5\,y + 2\cdot75\,(x+y) = 95\cdot21$; or,

$$9\cdot25\,x + 6\cdot25\,y = 95\cdot21 \ \& \ x = \frac{95\cdot21 - 6\cdot25\,y}{9\cdot25}$$

$\dfrac{139 - 10\cdot25\,y}{13\cdot25} = \dfrac{95\cdot21 - 6\cdot25\,y}{9\cdot25}$ by equating the two values of x.

From this we deduce $y = \dfrac{24\cdot22}{12\cdot1} = 2$ very nearly

Hence, . . . $x = 8\cdot94$

Consequently, the lime in 100 grains of mineral is 7 grains, and the carbonate of lime 12·5 grains.

The strontian in 100 grains of the mineral is 58·11 grains, and the carbonate of strontian 82·69.

Hence the constituents of the mineral are,

Carbonate of strontian, . 82·69
Carbonate of lime, . . . 12·50
Peroxide of iron, . . . 1·00
Zeolite, ... 3·79
—————
99·98

The peroxide of iron and zeolite being foreign matter, it is obvious that the pure mineral is a compound of

2 atoms carbonate of lime, . 12·5
9 atoms carbonate of strontian, 83·25
—————
95·75

It constitutes a new species of *calcareo-carbonate of strontian*, which we may distinguish by the name of *Emmonite*, from Professor Emmons to whom we are indebted for our knowledge of it.

This with the two species of carbonate of strontian described in my Mineralogy (vol. i. p. 107.) constitute the three following species,

1. Green carbonate, 10 Str $\dot{C}$ + Cal $\dot{C}$
2. Brown carbonate, 7 Str $\dot{C}$ + Cal $\dot{C}$
3. Emmonite, . . 9 Str $\dot{C}$ + 2 Cal $\dot{C}$ or
$4\frac{1}{2}$ Str $\dot{C}$ + Cal $\dot{C}$

ARTICLE IV.

Analysis of the Water of Nutshill Spring. By THOMAS
    THOMSON, M. D., F. R. S. L. & E., Regius Professor of
    Chemistry in the University of Glasgow.

THIS spring occurs at Barhead in the parish of Neilston,
situated about five miles south-west from Glasgow. It is
a chalybeate, differing in its constitution from all other
chalybeates that I have had an opportunity of examining.
In chalybeates the iron is sometimes held in solution by
carbonic acid, as is the case with the water of Tunbridge
Wells; the small quantity of iron in Bath water, is also
in all probability kept in solution by carbonic acid. In
such water the iron is always in the state of *protoxide*,
when it is sure to be most active. In other chalybeates
the iron is held in solution by sulphuric acid, in which
case the iron may be either in the state of protoxide or
peroxide. But I have most frequently found it in the state
of peroxide; and the quantity of salt of iron present in
such waters is often enormous. There are two chalybeates at
Moffat in Scotland. One of these the Hartfell Spa contains
36·75 grains of proto-sulphate of iron in the imperial gallon.
The other spring called the strong chalybeate, contains
591 grains of sulphated peroxide of iron in the imperial
gallon. But the strongest chalybeate of this kind which
I have ever met with, is that at Vicar's Brig about two
miles east from Dollar, and at the foot of the Ochil Hills
in Scotland. From the imperial gallon of this water (which
has a deep red colour,) I obtained

Common salt.	5·87 grains
Sulphate of soda	170·99
Sulphate of alumina . . .	953·18
Bi-sulphated peroxide of iron	1753·10
Sulphated peroxide of iron .	141·55
Silica	58·70
	3083·39

This is by far the strongest mineral water that I have
ever had an opportunity of examining.

Nutshill water is much weaker; but it owes its chaly-
beate properties to chloride of iron. Its taste is saline and

bitter, and its specific gravity 1·0121.   From the imperial gallon of this water I obtained

Chloride of iron	.	.	34·126 grains
Chloride of calcium	.	.	308·777
Common salt	.	.	620·614
Silica	.	.	1·333

$$\overline{964·850}$$

   In an imperial quart, which was all of this water that I had to analyze, I could detect no sulphate even when the liquid was concentrated to a tenth-part of its volume.   The imperial gallon contained two cubic inches of sulphuretted hydrogen gas.   The common salt extracted from Nutshill water contained a notable proportion of iodine—indeed a much greater quantity than I have met with in any other mineral water, that I have subjected to analysis.

ARTICLE V.

On the Connexion between Refracted and Diffracted Light.
By PAUL COOPER, ESQ.

(Read before the Royal Society, 8th May, 1834.)

(Concluded from page 355.)

THE difference in the breadth of the fringes, when the screen is placed at different distances from the object, as in the third observation, proceeds from the developement of the different colours by divergement, upon the same principle that the spectrum is formed upon a screen, when the light is refracted by a prism.   The rays of different colours which cross each other, and form white light at the edge of the object, open at this point at the same angle as that by which they approached it, and, of course, the farther the screen is removed from the object, the more the colours are separated from each other, and, consequently, the broader the fringes.

   The curved form of the line joining the same point of the fringe at different distances behind B, arises from the intersection of rays, the refraction of which, from having passed through different parts of the lens, is constantly changing with the distance; it is, therefore, correctly

2 E 2

stated, that the same fringe is not formed by the same light, at all distances from the body, but by the intersections of different rays.

This will be rendered more evident by the following considerations, with a view to the explanation of the fourth observation.

As all the rays of light which cover the triangular space F C D, cross somewhere near F, it necessarily follows, that their divergence is unequal; that the central ray must be unrefracted; and that from the line formed by it, the refraction must gradually increase to C and D.

If, then, we place an object upon the line R, at different distances from F, or *the focus of divergence*, its edges will be surrounded by light differently refracted; if, for instance, we place the object so near to F, that only the rays which fall on the screen near C and D, are allowed to pass, the boundaries of its shadow will be formed of the most highly refracted light; if the object is taken to B, its shadow will be bounded by rays which having passed through the lens near the central ray, are little refracted; if we take it to b, its shadow will be bounded by the intermediate rays, and be refracted accordingly. In all these cases, the fringes indicate the degree of refraction by their different breadths. M. Fresnel has given the result of some experiments on this subject, from which it appears, that when the distance of the object was four inches from the focus F, the angular inflexion of the red rays of the first fringe was $12' 6''$; and when the distance was increased to twenty feet, the angular inflexion of the corresponding rays was only $3' 55''$; the screens, C D, c d, which receive the shadows, being, in both experiments, placed at 39 inches from the object. The breadths of the fringes, in these experiments, vary at different distances, from the focus, as they would in surfaces of light refracted by prisms of different refracting angles.

The fringes commence with the most refrangible rays reckoning from the shadow.

The light, which at the moment of its passing the edge of the object, forms white light, is composed of rays transmitted by different parts of the lens, the least refrangible colours of which meet at this point, wherever it may be

situated, with the more refrangible colours which proceed from a part of the lens nearer its centre; the greater refraction of the former, compensating for the greater refrangibility of the latter, so as to bring the rays of the different colours to the same point.

This will appear more evident, perhaps, if we suppose the object B to be fixed on a part of the central ray corresponding in distance with a certain part of the lens, so that, supposing light to be equally refrangible, the light from this circle of the lens would pass the edges of the object, and form the circle of light nearest the shadow; and then, removing the condition of equal refrangibility, in conformity to the known law of refraction, consider, that at whatever distance from the centre of the lens this circle may be situated, the more refrangible rays belonging to it, from their greater divergence, cannot possibly arrive at the edge of the object, with the less refrangible rays, so as to perform the conditions required; for either the latter must fall upon the object, and be intercepted, or the former must be at such a distance from the edge as to allow other rays to pass from circles nearer the centre. In either case the light of different colours, which passes the edge of the object to form the fringes, must consist of rays, the most refrangible of which proceed from a part of the lens nearer the centre, than the less refrangible rays which meet them at this point; and as they cross here, and open at the same angle as that by which they approached, the more refrangible rays, which were previously outside, will now be inside, or nearest the shadow: every fringe is, in fact, the boundary of the shadow of the object, in the light of the colour in which it is seen.

I do not mean to deny the action of the interposed body upon the light which passes its edges; but in the present experiments, the resulting appearances are in a great degree independent of it, the fringes being in every respect the same, according to the experiments of Sir Isaac Newton and Sir David Brewster, whatever may be its density or refractive power.

Hitherto the developement of the theory has produced a literal explanation of the facts; but we have only yet accounted for one set of fringes; and if the theory has im-

posed any difficulty upon us, it is in explaining the appearance of three orders of fringes, at certain relative distances from each other.

The light we have been treating of is so constituted, that wherever the continuity of its surface is broken, the interval produced by the interruption, as the light proceeds, will be fringed with colours, which will increase in breadth by increase of distance until the whole space is covered.

Now, supposing we were at liberty to adopt the hypothesis that bodies are surrounded by alternate spheres of inflexion and deflexion, at different distances from their surfaces; and then assume that the spheres of inflexion were situated at distances corresponding with the intervals between the fringes, it would be a complete solution of the the difficulty; fringes would be formed upon the opposite edges of the intervals, complementary to each other, which before their complete developement, would be separated by the white light observed by Grimaldi; and the light inflected, carrying with it its divergent state, would form the colours observed by him within the shadow.

But there are several objections to this explanation, and a very principal one is, that the existence of these spheres of inflexion and deflexion has, never been proved; and it is evident, they are assumed for the purpose of giving a plausible explanation of appearances which, it appears to me, may be much better accounted for upon the known principles of refraction.

Before we proceed with this part of the subject, it is desirable to inquire more particularly, by what force, or by what means, light is diffracted by passing through small apertures. The principle upon which this light is formed, when it is produced by passing through a lens, has been sufficiently explained, and a little consideration will render it evident, that light which passes through a small round aperture, is diffracted by the same force, differently applied, but so as to produce a precisely similar arrangement.

It is generally admitted that the refraction of light commences at some little distance from the new medium, and continues through the same distance within it; increasing from nothing to a maximum as it enters the medium, and

then decreasing in the same gradual manner, until the light commences its rectilineal course, at an equal distance from the surface within the medium.

The refractive force, then, within the sphere of refraction, decreases as the distance from the surface of the refractory body increases, and this might naturally be expected; when, therefore, light passes through a small round aperture, the central ray, whether beyond the sphere of refraction, or under the influence of equal and opposite forces, must proceed in its rectilineal course; and the light surrounding it, must be gradually more and more refracted as it recedes from it, until, upon its passing close to the edge of the aperture, its divergence arrives at its maximum.

The arrangement which must be thus produced, is so precisely similar to that produced by a lens, as already described, that we are justified in concluding the refracting and diffracting forces are identically the same; and that the difference arising from the different applications of it, proceeds from the light passing through the whole sphere of the refractive force, when it enters the medium, and only through a part of it, varying in degree with the distance, when it merely passes the edges; in the first case the light is uniformly refracted, in the last it is refracted in proportion to the force to which it has been exposed.

The following experiment corroborates these statements :—

If we place an object with a straight edge, nearly close to the eye, and when at some distance from a window, bring the edge parallel to one of the bars that separate the panes of glass, so that the light from both sides of it, in the transmission to the eye, may pass close to the edge of the object, we shall perceive fringes of colours on each side of the bar; if it be a horizontal bar, and we look *over* the edge, the fringes above the bar will be red and yellow, and below the bar blue and violet, similar to what would be produced by a prism with a very small refracting angle, held upward; and if we look *under* the edge, the fringes above the bar will be blue and violet, and under the bar red and yellow, similar to what would be produced by the same

prism with the refracting angle downward. In the former
case, the light, and of course any object illuminated by it,
appears elevated, and in the latter case depressed, as it
would in passing through a prism, held as we have de-
scribed; and the different images being differently ele-
vated or depressed, exhibit fringes, where the colours are
separated.

The action of bodies upon light, which passes within the
sphere of their refractive force, being proved, not only by
the formation of diffracted light, but also by other experi-
ments, we cannot suppose that bodies when interposed in
such light, are divested of this influence as it regards the
general direction of the rays, though it may not otherwise
be necessary to the developement of the fringes.

This action and its character is exhibited in the follow-
ing experiment:—

If we place the edge of an object near the eye, and look
past it to a candle at some distance, so as to cut off a nar-
row vertical line of light, the light will be variously
refracted at different distances from the object, until they
are lost in the surrounding light. If we remove the ob-
ject to a greater distance from the eye, the images will be
reduced in breadth, so as at length to be scarcely distin-
guishable; and if, when they are thus reduced, we bring
the edge of another object into such a position, that the
light, after passing the first object, must pass within the
refractive sphere of the second, and between the two
objects; the images will again appear on the edge of the
first object broader, and more distinct than at first, and
there will be a very evident inclination of the light towards
the second object.

In this experiment, the light diffracted by the first object,
passes the edge of the second, which acts upon it in the
same manner as when an object is interposed in light, dif-
fracted by the usual process; and we have thus ocular
demonstration of the action of the interposed object, with
an evident proof that it draws the light which passes within
the sphere of its refractive influence towards itself.

When light is diffracted by passing near the edge of
one body, and then passes near the edge of another
body, in an opposite direction, in the manner we have

just described, it is not restored to its former state, as it would be in passing through a second prism of equal refractive power, in an opposite direction; because in the former case, with the usual arrangement, as well as in the present experiment, the light least refracted by the first object, is least exposed to the highest degree of refraction from the second; and this must necessarily produce the separation of the light into distinct images in the experiment, with the candle, and produce those intervals in the other experiments, which lead to a developement of the fringes.

Many other appearances, which have hitherto been among the mysteries of the science, may be traced to the same principle; the unequal refraction of light, before unequally refracted, and indebted for its colourless state to a system of mutual compensations, must frequently produce colours, either directly, by disturbing the order of the arrangement, or indirectly, by producing intervals for the developement.

The dark lines observed when light is admitted through a narrow aperture, may be accounted for upon this principle; and when the light, thus admitted, is dispersed by subsequent refraction, these lines ought to appear in the different colours of the spectrum; but if they are the same lines which have been observed by Fraunhoffer, and other philosophers, how they became fixed, is a question which the present state of our knowledge has not prepared us to answer: the fact is unconnected, by analogy or otherwise, with any known phenomena to aid us in its explanation.

The refraction of common light is always accompanied by partial reflexion; and as the attraction of the edges of bodies, or the refraction arising from it, accounts for the inflexion of a part of the transmitted light towards the body, so the attendant reflexion accounts for the deflexion of another part from it; and we may thus account for the principal phenomena of diffracted light, without the assumption of forces, unconnected with any other object, and without deviating from the usual simplicity of nature.

I am fully persuaded that the effects of refraction, traced upon known and admitted principles, are quite sufficient to account for the greater part of those appearances which

have been attributed to interference; and though I am
not prepared to deny the existence of this principle alto-
gether, provided it does not extend to destructive interfer-
ence, I am disposed to defend nature against the charge
included in it, of defeating her own purposes by the means
she has adopted for executing them. Her resources are so nu-
merous, and in tracing her operations, spring up amidst the
most complicated appearances, with such unexpected sim-
plicity, that I cannot admit a principle that implies a doubt
of the perfect adaptation of her means to their object,
without the clearest proof; and I am the more disposed to
hesitate when called upon to make this admission, from a
conviction that it has been claimed, in several instances,
to dispose of difficulties which may be removed by other
and better explanations.

It appears to me that the doctrine of interference, if it
was consistently followed up, would deprive us of light
altogether; for as light is supposed to flow from every point
of the sun to every part of space, whatever may be the
length of the undulations, they must be intersected by other
undulations, which originate at such a distance from them
as to make a difference of half an undulation in the length
of their paths, and according to this doctrine, if I correctly
understand it, the whole ought to be destroyed; whereas,
notwithstanding the millions of such intersections which
must take place in the course of its progress, light reaches
the eye unimpaired by its transmission.

<div align="right">P. COOPER.</div>

Bawlish, January 8th, 1836.

<div align="right">*To the Editor of the Records of General Science.*</div>

ARTICLE VI.

On Donium, a New Substance discovered in Davidsonite.
By Mr. Thomas Richardson.

This mineral was discovered by Dr. Davidson, of Aberdeen,
in a granite quarry, in the neighbourhood of that city, and
sent by him to Dr. Thomson, who requested one of his
pupils to submit it to a chemical examination, from which
he concluded it to be a compound of

Silica	.	.	.	.	.	66·59
Alumina	.	:	.	.	.	32·12
Water	.	.	.	.	.	1·30

Calculating the atomic proportions of these constituents, he found it to be,

Silica	.	.	.	.	.	2⅓ atoms
Alumina	.	.	.	.	.	1 atom*

The extreme improbability of the existence of such a compound, induced him to request me to make another analysis of this mineral.

For this purpose, 30 grains of the mineral were reduced to a fine powder, and fused with 60 grains of carbonate of soda. The fused mass was treated with dilute muriatic acid, and the silica separated in the usual way. It was collected on a filter, washed and dried, and found to weigh 13·49 grains, or 67·45 per cent.

The liquid which passed through the silica filter, was concentrated on the sand bath, and nearly neutralized with carbonate of ammonia. Caustic ammonia was then added in excess. A white precipitate appeared, which was separated by a filter, and washed. During the evaporation of the washings, a copious deposition of white flocks took place, as the ammonia was disengaged by the heat. This precipitate was dissolved in muriatic acid, and the evaporation continued. When sufficiently concentrated, oxalate of ammonia was added, but no precipitate appearing, the liquid was evaporated to dryness, and the dry mass heated to redness. The residue was dissolved in very dilute muriatic acid, and the solution boiled in a flask, with carbonate of soda. A white flocky precipitate appeared, the colour of which, during the boiling, changed to brown.— It was separated by a filter, and washed.

The precipitate which was obtained by caustic ammonia, though quite white when originally thrown down, had, during the washing, gradually acquired a brown colour. This precipitate being supposed to contain peroxide of iron, it was dissolved off the filter with weak muriatic acid, and boiled for some time with caustic soda. The soda threw down a white precipitate, which, after a short boiling, almost entirely disappeared, leaving only behind a small quan-

* Thomson's Mineralogy, i 247.

tity of a dark brown flocky matter. This being supposed to be iron, was separated by a filter.

The caustic soda solution was saturated with muriatic acid, and the white matter again thrown down, by carbonate of soda. This precipitate was also collected on a filter, and during the washing, the colour changed to brown.

The brown matter, insoluble in caustic soda, which was supposed to be peroxide of iron, was dissolved in muriatic acid. Caustic ammónia being added, a white precipitate appeared, which was completely dissolved in an excess of the precipitant.

There were several circumstances in this analysis, which appeared to indicate that the mineral contained some other base besides alumina. The striking solubility of the first precipitate in caustic ammonia, showed that it could not consist entirely of that earth; while the remarkable fact, that all the precipitates, though at first white, became brown during the washing, rendered it probable, that they all contained at least a portion of the same substance.

From these peculiarities, it was considered necessary to repeat the analysis on a larger scale; and in order to fix upon a method of proceeding with it, a few experiments were made, to determine if possible, the nature of the constituents. For this purpose, a portion of the brown matter was dissolved in muriatic acid, and the solution rendered as neutral as possible, by evaporation. The properties observed were the following :—

1. When a current of sulphuretted hydrogen gas was passed through the solution, previously rendered slightly acid, a light brown precipitate appeared.

2. When an excess of caustic ammonia was added to the solution, through which the sulphuretted hydrogen had been passed, the precipitate became much more dense, and of a dark green colour.

3. When caustic soda was added to the solution, a white flocky precipitate appeared, which was readily dissolved in an excess of the precipitant.

4. Caustic ammonia also threw down a white precipitate, equally soluble in an excess of the alkali.

5. Carbonate of ammonia acted in the same way.

These characters rendered it perfectly evident, that this

mineral contained a substance quite different in its properties from alumina, or any other of the earthy bases, and the most obvious method of separating it from any of these latter, which might occur along with it, appeared to be by means of sulpho-hydrate of ammonia.

A. 100 grains of the mineral were finely pounded, and fused with 300 grains of carbonate of soda. The fused mass was treated in the usual manner, and the silica separated by a filter.

B. To the concentrated solution and washings from (A), sulpho-hydrate of ammonia was added.' A very bulky dark green precipitate fell, which was filtered and washed.

C. The liquid which passed through the filter (B), was concentrated, and oxalate of ammonia added. No precipitate appearing, the evaporation was continued to dryness, and a very small quantity of magnesia, obtained in the usual way.

It appears from this analysis, that if we except the minute portion of magnesia, the mineral is composed entirely of silica, and the substance precipitated by sulpho-hydrate of ammonia. The next point was to determine what this substance was, and as its distinguishing character seemed to be its solubility in caustic alkalies, the sulphuret was decomposed by nitro-muriatic acid, and treated with caustic ammonia in a well-stoppered bottle, which was occasionally agitated, to promote the solution. The digestion was continued for a fortnight, and the clear liquid drawn off, at first every 24 hours, and latterly once in two or three days. The precipitate, during the continuance of the digestion, gradually assumed a reddish brown colour, and its solubility evidently diminished as the colour deepened, till at last the quantity dissolved by the caustic ammonia became so very small, that it appeared useless to carry the digestion any further. The portion remaining undissolved, which amounted to nearly one half of the whole, was therefore again dissolved in muriatic acid. To this solution, caustic soda was added in excess, and a white precipitate fell, which, when the liquid was boiled, partly dissolved, and partly assumed a brown colour. The whole was thrown upon a filter, which collected the brown matter, while a colourless solution passed through.

The green precipitate thrown down by sulpho-hydrate óf ammonia, was thus separated into three portions:

A. That which was dissolved in caustic ammonia.

B. That which was dissolved in caustic soda.

C. The undissolved brown portion, which was separated from the caustic soda solution.

A. The caustic ammonia solution was evaporated to dryness, and the dry mass heated to redness in a platinum capsule. The residue was a very light white powder, which possessed the following characters:

I. Before the blowpipe.

Per se. It is not altered.

With carbonate of soda in the exterior flame, it tinges the bead of a green colour, which changes to pink in the interior.

With borax, it fuses into a transparent colourless bead.

With salt of phosphorus it fuses into a transparent bead, which is yellowish while hot, but which becomes colourless on cooling.

With nitre in the exterior flame, it forms a mass of a blue colour, which undergoes no change in the interior.

With a solution of nitrate of cobalt, the substance is tinged of a purple colour, which is very deep at first.

II. This white substance dissolves in nitric and muriatic acid, and with difficulty in sulphuric.

III. The solution in muriatic acid, concentrated and set aside, deposited very minute crystals, which appeared to be four-sided prisms, and when evaporated to dryness, the residual fall had a bright yellow colour, a sweet astringent taste, and deliquesced when exposed to the air.

IV. The same solution slightly acid, behaved in the following manner with re-agents:—

1. With caustic ammonia—gave a white flocky precipitate, soluble in excess of the precipitant.

2. With caustic soda—it acted in the same way.

3. With carbonate of ammonia—also in the same way.

4. With carbonate of soda—white flocky precipitate, insoluble in excess of re-agent.

5. Sulpho-hydrate of ammonia—flocky green precipitate.

6. Oxalate of ammonia—no precipitate.

7. Phosphate of ammonia—curdy white precipitate.

8. Arseniate of soda—white flocky precipitate.
9. Sulphate of soda—no precipitate.
10. Chromate of potash—copious yellow precipitate.
11. Tartrate of potash—slight white precipitate.
12. Infusion of nutgalls—no precipitate.

B. The caustic soda solution was mixed with a solution of carbonate of potash, and sulphuric acid added in slight excess. When this solution was sufficiently concentrated by evaporation, it was set aside to crystallize. After standing twelve hours, a copious deposition of crystals was observed. These consisted in a great measure of large prisms of sulphate of soda; but with these a number of small octahedrons were mixed, some of which had the edges and terminal angles truncated, while others were quite entire. The whole was set aside in a dry place, till the crystals of sulphate of soda had effloresced, and fallen to powder, after which the octahedrons were easily separated.

From experiments made with them the following characers were deduced:

They dissolve easily in water.

They do not effloresce when exposed to the air.

When heated they undergo the aqueous fusion, swell up and become white and opaque.

Their taste is sweet and astringent.

In a solution of these crystals, chloride of barium produces a copious white precipitate, insoluble in excess of acid.

Caustic ammonia produces in the solution a white precipitate, which is immediately dissolved in excess of the precipitant.

Chloride of platinum causes a heavy yellow precipitate.

This salt is therefore composed of sulphuric acid, white substance, potash and water.

C. A portion of the reddish brown matter, which remained undissolved in caustic soda, was dissolved in muriatic acid, and caustic ammonia added. A white precipitate appeared, which was immediately dissolved, when the ammonia was added in excess. There could be no doubt, therefore, that this substance was the same in its nature as the former. It was cautiously dried on the sand bath. When dry, it had a buff colour, which appeared to be slightly deepened, when it was heated to redness. The

dry matter, when treated with dilute muriatic acid, effervesced, but did not dissolve, and its colour was changed to white. After ignition, it was found to have a specific gravity of 4·309.

The substance which has thus been obtained from Davidsonite, and examined in detail, appears to be possessed of properties different from any hitherto known. From the alkaline and earthy bases, and from several of the metallic ones, it is eminently distinguished by the green precipitate which it gives with sulpho-hydrate of ammonia; while its solubility in the caustic alkalies, and in carbonate of ammonia, the light brown precipitate thrown down by sulphuretted hydrogen, and the green given by sulpho-hydrate of ammonia, are amply sufficient to distinguish it from all the others.

If this substance be considered as sufficiently distinct, which, from its characters, I think I am warranted to conclude, I shall propose to give it the name of *Donium*, being a convenient contraction of Aberdonia, the Latin name of Aberdeen, near which place Davidsonite occurs; for the suggestion of which name I am indebted to Dr. Thomson.

The change of colour which the precipitates of this substance undergo, during the process of washing, appears to be owing to different degrees of oxidation; and with a view to determine, if possible, the characters of the metal itself, as well as its degrees of oxidation, the following experiments were made:

A. Over a portion of the white oxide strongly heated to redness, in a green glass tube, a current of dry hydrogen gas was passed, for nearly an hour. The whole was converted into a slate-blue mass, while aqueous vapour was evolved at the end of the tube: 100 parts of the white powder, by this means, lost 16·34 of their weight.

B. A portion of the buff oxide was treated in the same way, and the same slate-blue powder was obtained, with the evolution of aqueous vapour: 100 parts of this oxide lost, by this process, 5·11 of their weight.

The substance possessing the slate-blue colour exhibited the following characters:

1. When pounded in a dry agate mortar, it appeared to assume a lustre, resembling the metallic.

2. When heated to redness, it glowed like tinder, and became white.

3. In dilute muriatic acid, it effervesced, and was converted into a white powder.

4. When placed in a charcoal crucible, properly enclosed, and heated strongly in a forge for half an hour, it was not altered.

It seems probable, that the slate-blue substance consisted of metallic Donium, but in a state of intimate division; while from the experiments made upon the oxides, upon which, however, for many reasons, great confidence cannot be placed, it would appear that the oxides are composed of

1. The Buff . . 94·89 Donium + 5·11 oxygen,
2. The White . 83·66 Donium + 16·34 oxygen.

Or, that the white oxides contains thrice as much oxygen as the buff.

Although circumstances do not permit of my continuing this investigation, I have reason to believe that it will not be laid aside, but that a more full account of this substance will shortly be given by an individual much more capable of performing the task.

ARTICLE VII.

On a difficulty in Isomorphism, and in the received constitution of the Oxygen Salts; in a Letter to Professor Mitscherlich, of Berlin, from THOMAS CLARK, M. D., *Professor of Chemistry in Marischal College, Aberdeen.**

SIR,—I do myself the honour of addressing to you some observations tending to remove, in your doctrine of Isomorphism, a discrepancy arising from a fact stated in your valuable paper on the Acids of Manganese. The proposed observations have an immediate object that happens to be of the greater interest, inasmuch as they cannot accom-

* Dr. Clark uses the Continental symbols and atomic weights, omitting minute fractions. But chlorine, instead of *Cl.* he makes Ch.; Sodium, *So.*; Silver, *Sv.*; on account of his considering their weights as double those used on the Continent.—EDIT.

plish that object, without going far to modify our concep-
tions of Oxygen-salts and Oxygen-acids.

In various of your papers illustrative of Isomorphism,
you have proved that sulphur, selenium, chrome, and man-
ganese, have the power of re-placing each other, in com-
pounds, without affecting the resulting form, and that sil-
ver, in certain compounds, is re-placed, in like manner, by
sodium. The evidence that these conclusions rests upon,
cannot be better shown to be stable, than by putting it in
array. The salts in each of the four columns of the follow-
ing table, are like each other in form, but different, in that
respect, from any of the salts contained in the three other
columns.

	of Potash.	of Soda ($\dot{N}$a). / of Silver (Ag.)	of Soda with Water.	of Silver with Ammonia.
Sulphate ..	$\dot{K}\ddot{S}$	$\dot{N}a\dot{S}$ $\dot{A}g\ddot{S}$	$\dot{N}a\ddot{S}$ +10H$\dot{H}$	$\dot{A}g\ddot{S}$ +4NH
Seleniate ..	$\dot{K}\ddot{S}e$	$\dot{N}a\ddot{S}e$ $\dot{A}g\ddot{S}e$	$\dot{N}a\ddot{S}e$ +10H$\dot{H}$	$\dot{A}g\ddot{S}e$ +4NH³
Chromate .	$\dot{K}\dot{C}r$		$\dot{N}a\dot{C}r$ +10HH	$\dot{A}g\dot{C}r$ +4NH³
Manganate	$\dot{K}\ddot{M}n$	———	———	

In the foregoing table, the substituting power of manga-
nese has least of evidence to support it, in as far as this
metal occurs therein, as an element, only once.   But what-
ever want of evidence may be supposed to arise hence, is
well supplied in the two following salts, alike in form and
constitution :—

Ammoniacal chrome alum . . $\dot{A}m\ddot{S} + \dot{C}r$ $\ddot{C}r\ \ddot{S}^3$ + 24H$\dot{H}$

Ammoniacal manganese alum $\dot{A}m\ddot{S} + \dot{M}n$ $\ddot{M}n\ddot{S}^3$ + 24HH
            (Where Am is ammonium $= N^2\ H^8$).
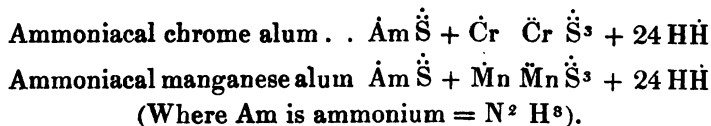
Now the discrepancy I have alluded to is this—

    The waterless sulphate of soda   . . $\dot{N}a\ddot{S}$

and of course, the isomorphous salts $\dot{A}g\ \ddot{S}$,

$\dot{N}a\ddot{S}e$, $\dot{A}g\ddot{S}e$, you found to be of like form,

Not with manganate of barytes . . . Ba M̈n

But with oxymanganate of barytes . . Ba M̈n M̈n

This discrepancy, which, occurring *in such a case*, appeared to me very startling on the first perusal of your paper, I propose to show, may be removed, by regarding the salts in question, as we may reasonably do, notwithstanding our preconceived notions to the contrary, to be as much alike in constitution, as you have proved them to be in form.

That you may the better judge of the ground I proceed on in offering such a suggestion, I may advert previously to a change, similar, in its reasons, to the one I am going to suggest—a change, however, which the notions of chemistry prevalent on the Continent do not admit or call for, but which your paper is calculated to bring about in regard to one point of doctrine, on which the British chemists differ from the Continental. While, by both, the atoms of oxygen, potassium, sulphur, and manganese, are held at proportional weights, the atom of chlorine is held by us at double the weight it is held by you. Between these two views of the atomic weight of chlorine, the choice, I apprehend, must be decided by your discovery, that the oxymanganate of potash corresponds in form with the oxychlorate. Representing these compounds in their *ultimate* components, we have, according to

Your view and ours, K + 8 O + 2 Mn oxymanganate of potash

Yours . . . . K + 8 O + 2 Cl $\Big\}$ oxychlorate of potash
Ours . . . . K + 8 O + Ch

(Ch, representing our atomic weight of Chlorine, being double Cl, representing your weight of the same atom.)

Your doctrine of Isomorphism—that is, THE FACT ascertained in other instances, that, in compounds different in some of their components, but agreeing in the number of the atoms of those components, the resulting form is often the same—this doctrine comes in between those two views, as a witness on behalf of nature, to enable us to decide which is true. The doctrine itself, indeed, I am well aware, is little heeded by some chemists, who seem therein swayed chiefly by a certain indolence that hinders them from letting their attention dwell on difficulties, existing, no doubt,

and to be looked for, in the early history of a discovery so pregnant with consequences. But there is a quality of iso-morphous bodies that should arrest the attention of any chemist, however devoted to the details and practice of chemistry, and however averse to speculation. It is not, that compounds differing in their components, but alike in constitution and form, approach each other so near in pro-perties, as the phosphates and the arseniates, or as the various individual alums are found to do, discordant as at least some of such compounds manifestly are in the cha-racter of their ultimate components; but it is, that com-pounds, when alike in constitution and form, although different in elements, have, as was first established many years ago by Beaudant and Wollaston, the property of crystallizing together *in proportions that are indefinite*, yet in such a manner as to produce crystals, perfect in their form, and as clear throughout their mass, as if they were pure and unmixed, and with so little disturbance to the harmony of the mixed compounds, as, whenever they hap-pen to be of different colours, so to blend these as to pro-duce tints, corresponding in depth to the proportions of the various coloured compounds that make up the crystalline mixture.* Strange it is, to find slighted in importance, the triumphant doctrine that, in achieving this discovery, overthrew the last obstacle to the establishment of definite proportions, and laid open to the chemist the mystery of the mineral kingdom.

While abiding by this your doctrine, and proceeding on a like principle to what has just now been illustrated in the case of the oxymanganate and the oxychlorate of potash, it is possible, I conceive, to remove that unlikeness of con-stitution, so apparent in the two following salts of like form :—

The oxymanganate of barytes   .   .   . $\overset{.}{Ba}\ \overset{..}{Mn}\ \overset{..}{Mn}$

The waterless sulphate of soda .   .   . $\overset{.}{Na}\ S$

These two salts, we may better compare, unswayed by any theory, by regarding them in their ultimate components, thus :—

* This is the view taken of isomorphism by Dr. Thomson. See *Inorganic Chemistry*, i. 21.—EDIT.

$$\dot{B}a \, \hat{M}n \, \overset{..}{M}n \, = \, Ba + 8\,O + 2\,Mn$$

$$\dot{N}a \, \overset{.}{S} \quad\quad = \, Na + 4\,O + \; S$$
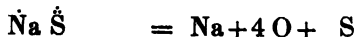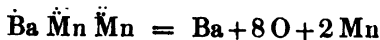
But, as manganese is isomorphous with sulphur, we can, better still, compare the two salts by taking the ultimate components of two atoms of sulphate of soda, thus :—

$$\dot{B}a \, \hat{M}n \, \overset{..}{M}n \, = \, Ba + 8\,O + 2\,Mn \; \text{oxymanganate of barytes.}$$

$$2 \; \dot{N}a \; \overset{.}{S} \quad = 2Na + 8\,O + 2\,S \; \text{Sulphate of soda.}$$
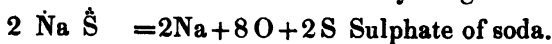
Instead whereof I suggest,

$$So + 8\,O + 2\,S \quad ,, \quad\quad ,,$$
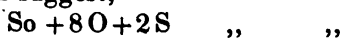
(Where So stands for the atom of sodium, being, in weight, double the received atom, which is represented by Na.)

Comparing together, as here we do, so much of each of the salts as contains eight atoms of oxygen, we find two atoms of manganese substituted, without affecting the form of the compound, by two atoms of sulphur. This is like substitution to what you have shown takes place in the manganate and the sulphate of potash. Manganese and Barium, in the proportion here assumed to be that of atom for atom, may be regarded as possessing a like power of substitution, since, taken in that proportion, and in succession, manganese, iron, calcium, barium, have been found to re-place each other in compounds, without causing an alteration in form. Therefore, we are sure that, in the oxymanganate of barytes, the manganese is to the barium in the proportion of two atoms to one. Again, the relation of the atoms of sodium and silver, in the proportional weights generally assigned to these metals, may be regarded as demonstrated by your discovery, that, in compounds, these metals, in such proportions, may re-place each other, without any change in the form. But I am aware of no sufficient proof, heretofore adduced, establishing the relation of sodium or silver, in respect of atomic weight, to manganese, to sulphur, to oxygen, or to barium, in such a way, at least, as to forbid our either doubling or halving the received atomic weights of sodium and silver. Thus, in the oxymanganate of barytes and the sulphate of soda, while regarding the atomic weight of manganese as fixed, we cannot doubt that the atomic weight of barium should neither be halved nor doubled, but we are quite at liberty

to inquire, whether the atomic weight of sodium should be halved or doubled? So that, in regard to these two salts.

First, We find, in the one, two atoms of manganese, in the other, two atoms of sulphur— bodies proved to be isomorphous.

Second, We find eight atoms of oxygen in each.

Third, Therefore, the question to be resolved shrinks into this one point: In two salts of a similar form, and otherwise composed alike, are we to regard barium, which is in the proportion of *one* atom, as being substituted by sodium, in the proportion of *one* atom, or in the proportion of *two* atoms?

Now if, as I think, justly, we regard

The oxymanganate of potash, as containing $K + 8 O + 2 Mn$
And the oxychlorate, as containing   . . $K + 8 O + 2 Cl$
And not (assuming $2 Cl = Ch$) . . . . $K + 8 O + Ch$
How can we hesitate in considering
The oxymanganate of barytes, as containing $Ba + 8 O + 2 Mn$
And waterless sulphate of soda, as contng. $So + 8 O + 2 S$
And not   . . . . $2 Na + 8 O + 2 S$

Without renouncing, or at least, disregarding the known analogies of chemistry, I own I do not perceive how to evade the conclusion, that sodium and silver should each have its atomic weight doubled, and that the oxymanganate of barytes is not only similar in form, but analogous in composition, to the waterless sulphate of soda, and, of course, to the waterless seleniate of soda, and to the sulphate and the seleniate of silver.

But, even in two salts of a like form, we cannot regard analogy as fulfilled, merely by their containing in each a like number of corresponding ultimate components, partly of the same kind, and the rest isomorphous. Those ultimate components may form intermediate compounds, which, and not elementary bodies that are mere ultimate components, may be the real constituents of the salts. Besides, in isomorphous compounds, we should look, not so much for analogy of composition, as for analogy of constitution. When scrutinized for this purpose, the salts in question present a difficulty that chemists can overcome, as far as I can see, only by taking up a position, less repugnant to reason, than it may be to their habits of thinking.

Every chemist is aware, that the prevailing opinions respecting the constitution of oxygen salts—that sulphate of soda, for instance, consists of sulphuric acid and soda—were adopted long before the elements of such salts were known to the extent that they now are. We are old enough to remember how much the ideas of chemists were staggered at the discovery of the metals potassium and sodium, especially when it was found that common salt, while it could readily be proved to contain sodium, could not, by any device of ingenuity, or industry of investigation, be proved to contain soda. However much habit may since have worn off surprise, the published researches of the ablest chemists of the time evince, that the discovery of those metals was conceived to be little consistent with what were then, and what with many still are, the received notions respecting ammonia and the ammoniacal salts. The extravagant assumption of a combination so improbable, ideal, and untangible, as what was called the hydro-chlorate of soda, was but a proof, that chemistry had, on a sudden, outgrown the old garment of theory, that had once afforded to the whole body of its facts, a fit and ample covering, but which was now so inadequate that it could not be drawn over the nakedness of any one part, without making bare some other.

Subsequent researches, belonging more definitely to our own time, seem to have had more effect in unsettling former doctrines respecting the salts, than in establishing any new doctrine. But the opinions entertained by chemists respecting the constitution of oxygen salts appear, to be chiefly two. In the instance of sulphate of soda, one of these opinions regards the basis as the alkali soda ($\dot{N}a\, \dot{S}$), the other as the metal sodium ($Na\, \overset{..}{S}$). When the basis of the salt is conceived to be soda, oil of vitriol must be regarded as a compound of sulphuric acid and water ($H\dot{H}\, \dot{S}$); but when the basis of the salt is conceived to be sodium, the same liquid must be regarded as a hydrogen acid ($H^2\, \overset{..}{S}$). Whatever uncertainty there may be as to which of these views is to be preferred, none exists, in my mind, as to the constitution of the ammoniacal salts, so far at least as re-

gards the portion of them that corresponds with potassium in the analogous compounds containing this metal; for that potassium is, in every such compound, substituted by ammonium ($N^2 H^3$), I hold to be a point as well established as any within the range of ascertained chemistry. Quite consistent with such a view of the ammoniacal salts, however, is either of the forementioned opinions respecting the oxygen salts. The recommendation of simplicity and analogy belongs to the newer opinion, which regards the basis of Glauber's salt to be the same as that of common salt, the basis of nitre to be the same as that of sulpho-cyanide of potassium, the basis of green vitriol to be the same as the basis of what used to be called green muriate of iron. More especially does this appear, when, on adding tartaric acid to solutions of salts having so much resemblance as nitre and the sulpho-cyanide of potassium, we obtain cream of tartar in such case, or when, on adding caustic potash, or sulphuret of potassium, or yellow prussiate of potash, or the red prussiate, to solutions resembling each other, in colour, taste, and in action on the air, as do solutions of proto-sulphate, and proto-chloride, and proto-sulphocyanide of iron, we produce, by each re-agent, a like precipitate from all these three solutions; for surely it is hard of belief, that such likeness of character and of action all proceeds from bodies having no likeness of constitution. Obstacles, however, are in the way of our adopting the newer opinion, were it only for the difficulties that must occur in the application of a new doctrine, in all its details, to the numerous compounds that any doctrine relating to salts must embrace. And, until some discovery be made, inconsistent with either opinion, both must remain open to the choice of chemists, like two roads leading to the same place, an old and a new, where the new way, although the straighter and the more level, is yet avoided by the concourse of travellers, as happening to be the rougher, and less agreeable and easy, from the single circumstance of its being new.

These things I recall on the present occasion, conceiving that, if the suggestion be well founded, that oxymanganate of barytes is analogous in constitution to the sulphate of silver, and to the waterless sulphate of soda (wherein is

•implied that the received atomic weights of sodium and sil-
ver must be doubled), then a point is established incon-
sistent with the older opinion respecting the constitution
of the oxygen salts and oxygen acids; so that, precluded
from that opinion, chemists, in reference to the newer
opinion, may be in the situation of travellers, who, finding
an old road shut-up, are compelled to proceed on the new
made road that lies before them, consoling themselves as
they go, by considering how soon usage will give it smooth-
ness.

The suggested analogy between the oxymanganate of
barytes and the two sulphates already named, is in part
consistent with the usual conception of oxygen salts and
oxygen acids, and in part inconsistent. So long as we only
compare the salts of soda among themselves, or the salts
of barytes among themselves, or even when we compare
such of the salts of barytes and soda as are produced from
the same acid, no discrepancy arises, as the following for-
mulas will make apparent:

Of barytes . . .	The sulphate . .	$\dot{B}a \; \overset{\centerdot}{S}$
	The manganate .	$\dot{B}a \; \ddot{M}n$
	The oxymanganate	$\dot{B}a \; \dot{M}n \; \ddot{M}n$
Of soda	The sulphate . .	$\ddot{S}o \; \ddot{S}^2$
	The manganate . .	$\ddot{S}o \; \dot{M}n^2$
	The oxymanganate	$\ddot{S}o \; (\dot{M}n \; \ddot{M}n)^2$
The sulphate of .	Barytes	$\dot{B}a \; \overset{\centerdot}{S}$
	Soda	$\ddot{S}o \; \overset{\centerdot}{S}^2$
The manganate of	Barytes	$\dot{B}a \; \overset{\centerdot}{M}n$
	Soda	$\ddot{S}o \; \dot{M}n^2$
The oxymanganate of	Barytes	$\dot{B}a \; \overset{\centerdot}{M}n \; \ddot{M}n$
	Soda	$\ddot{S}o \; (\dot{M}n \; \ddot{M}n)^2$

Here, in consequence of the atomic weight of sodium
being doubled, soda becomes a binoxide ($\ddot{S}o$), corresponding
with the peroxide of tin ($\ddot{S}n$), while barytes remains a
protoxide ($\dot{B}a$), corresponding with the protoxide of tin

(S̊n). Accordingly, the differences of constitution that appear in the foregoing formulas, between, on the one side, barytes, and on the other the salts of soda formed from the sulphate, and the manganate, and the oxymanganate of the same acids, are precisely such as would occur between the proto and per salts that these acids would respectively produce, when combining with the protoxide and the peroxide of tin. Thus far then of discrepancy, there is none.

But, on comparing

<div style="text-align:center">

The oxymanganate of barytes . . Ḃa Ṁn M̈n

And the waterless sulphate of soda . S̊o S̊²

</div>

discrepancy appears; for here we are presented with two salts, alike in form and in the number of their ultimate components, but having these components combined together so as to form intermediate compounds quite unlike in constitution. The base of the first salt contains *one* atom of oxygen—the base of the second, *two* atoms of oxygen; the acid of the first contains *seven* atoms of oxygen—the acid of the second, *six* atoms of oxygen. One of two alternatives, therefore, is forced on our adoption. Either retaining the foregoing formula for sulphate of soda, we must assimilate thereto the one for oxymanganate of barytes, or retaining the formula for oxymanganate of barytes, we must assimilate thereto the one for sulphate of soda. Now, can we adopt either alternative? You shall judge, after considering the following attempt:

I. Retaining the formula for the sulphate of soda, and adopting a corresponding one for the oxymanganate of barytes, we must likewise alter the formula for oxymanganate of soda; but the other formulas do not need to be altered. When thus modified, the list of the salts already given will stand as follows:

<div style="text-align:center">

Sulphate of barytes . . .	Ḃa S̈
Manganate of barytes . .	Ḃa Ṁn
Oxymanganate of barytes .	B̈a Ṁn²
SULPHATE OF SODA . . .	S̊o S̊²
Manganate of soda . . .	S̊o Ṁn²
Oxymanganate of soda . .	S̊o M̈n⁴

</div>

The constitutions here assigned to the several salts imply, among others, the three following suppositions :

1. The basis of the barytic oxymangate contains, not barytes, but the peroxide of barium, which has not in any other instance been ascertained to constitute the basis of a salt.

2. The basis of the alkaline oxymanganate is not soda, nor peroxide of sodium, but an unknown oxide of sodium, with a third more oxygen than the peroxide, and, atom for atom, a third more than even the acid with which it is combined.

3. The acid in each oxymanganate is not oxymanganic acid, but the manganic.

In the instance of the oxymanganate of barytes, the extravagance of such suppositions is a little mitigated, by our knowledge, that manganic acid is readily convertible into the oxymanganic, and that peroxide of barium is an obtainable compound, although not ascertained to be the basis of any salt. But analogy would extend like assumptions to cases where no such circumstances of mitigation would exist. For example, the oxychlorate of magnesia, whose formula would become

$$\ddot{M}\overset{.}{g} \; \overset{.}{C}l^2$$
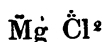
would present us with a basis that is not magnesia, but an unknown oxide of magnesium, and an acid that is not the oxychloric, nor even the chloric, but some other, not known, nor named.

In the instance of the oxymanganate of soda, it might, perhaps, be thought, that part of the difficulties that have been enumerated might be avoided by allowing its formula to remain unaltered—

$$\ddot{S}o \; \overset{.}{\ddot{M}}n \; \overset{..}{M}n$$

But, indeed, the difficulties would not thus be made fewer, they would be merely of a different character. By retaining this formula for the oxymanganate of soda, we would be involved in the inconsistency of admitting the barytic oxymanganate to contain the same acid as is in the manganate either of barytes or of soda, while we assume that the alkaline oxymanganate contains a different acid.

ARTICLE VIII.

The Art of Dyeing.

(*Continued from* page 385.)

GALLING THE CALICO.

THE application of the mordant to the calico commonly precedes what is termed galling. In this case, the calico, well bleached, but not mordanted, should be boiled with nutgalls, or other substances, containing tannin; and the calico, in this instance, takes up a certain quantity of gallic acid and tannin, and holds it so firmly, that washing the calico with water cannot separate the tannin from it.

The calico so treated should now be dipped in alum, or iron mordant; it then acquires determinate colours, according to the affinity of the matter employed in galling, and the mordant.

Other colouring matter also, as for example, yellow wood, logwood, madder, &c. unite in the same way with the calico, and then with the mordant.

In many printed colours, the galling becomes a principal operation, which must precede the impregnation with the alum mordant. I have not been able to convince myself of the utility of this practice.

It is certainly true, that a galled piece of calico, dipped in a solution of alum, takes up more alumina from it than an ungalled portion; but in this way, the great object, namely, dark, saturated, and clear colours is not obtained. This gives only acetate of alumina, in which galling is quite unnecessary. The moderate price of this mordant renders it of more general use at present, than formerly, where more alum must be employed.

Galling is employed for madder-dyeing.

A piece of yarn, first galled and then mordanted, becomes reddish brown, while yarn, impregnated with acetate of alumina, likewise dyed in the same solution with madder and bran, appears clear red, and requires a separate cleaning with soap and soda, in order to make them alike. Tannin does not act more favourably.

AFFINITY OF COTTON FOR DIFFERENT DYESTUFFS.

When calico, well cleaned and mordanted, is boiled with

madder, logwood, Persian berries, yellow wood, nutgalls, oak bark, willow bark, quercitron, &c., it takes up a certain quantity of each dye, which neither washing with cold or hot water, can remove. The calico will thus, when madder and yellow wood are employed, be faintly coloured, but when immersed in a mordant, especially in alum and lime water, becomes darker. Calico dyed in this way must be well boiled with ley and freed from all lime by means of sulphuric acid, otherwise spots will be formed.

Nutgalls.—When 20 lbs. cloth are boiled in a solution of 1 lb. nutgalls for half an hour, the former acquires a pale yellow colour; but dipped in a solution of 10 lbs. of iron alum, in 600 water, and rinsed in running water, the colour becomes greyish lilac.

The nutgall solution must be formed of a clear decoction of nutgalls. If powder of nutgalls is employed, spots remain after the calico is rinsed with water. Hot soap-suds impart to the colour a reddish tint, which looks well.

Properties of the dyed cloth.—The colour loses none of its depth, after boiling for a quarter of an hour; but is converted into a greyish brown.

Solution of potash produces brown spots, which are completely removed by vinegar.

Lime water acts in the same manner. If the cloth is moistened with lime water, it acquires a clear brown shade.

Solution of ammonia, much diluted with water, produces a similar effect: the colour, however, is less saturated.

Vinegar has no action.

Lime juice forms white spots, which ammonia turns brown. Both of the *Tin Mordants*, No. 1 & 2, when printed on it, discharge a pure white.

Solution of chloride of lime, in the proportion of one part chloride of lime to forty water, discharge white.

Nutgalls and copper, or lime water. When calico, boiled for half an hour in a solution of nutgalls, is well rinsed and dipped in the copper mordant, No. 2, or in lime water, a pale yellow colour is produced.

Properties of the cloth.—Cloth dipped in the copper mordant, acquires by boiling with soap-suds, a greenish tint; that dipped in lime water becomes somewhat paler.

Solution of potash induces in the former yellow spots, with greenish borders, which are removed by vinegar.

Lime water, ammonia, and *vinegar,* have no action upon either.

Lime juice produces on both white spots, removeable by ammonia.

The *tin mordants* discharge both white.

Solution of chloride of lime bleaches the printed places but imperfectly.

Yellow wood.—Calico, unimpregnated with mordant, when boiled for half an hour in a solution of yellow wood, acquires a straw colour. When dipped in a solution of iron, consisting of 10 lbs. iron alum, and 600 lbs. water, a light yellow is formed.

Remark.—When quercitron is boiled with unmordanted cotton, the latter acquires not a yellow, but a reddish colour, which will be but slightly tinged, and removed by iron alum, alum mordant and lime water.

When boiled in Persian berries, the calico acquires a gray lustre, which will be little changed by the mordants.

When boiled with willow and oak bark, the calico acquires a reddish colour, which will neither be altered, nor removed by mordants. Fernambuc colours the calico, pink.

Avignon madder gives a dark pink, which will be little altered by mordants.

Logwood.—The mode of dyeing and mordanting is that formerly described. When the water employed for dyeing contains lime, the solution of logwood should first be boiled and skimmed, and then be brought in contact with the calico previously well moistened.

Tannin and bablah.*—The application of these dyes is the same as that of the nutgalls. In this case, the clear solution of the tannin should only be employed for dyeing, otherwise spots remain when the cloth is dipped in the solution of iron alum. The properties of the cloth are the same as those given under dyeing with nutgall and iron alum. Specimens dyed with these colours exhibit strikingly what is meant by dyeing of one colour, or going into the ground. It proceeds from the colouring matter

* I am not sure of the meaning of this word.—*Trans.*

combining intimately with the unmordanted calico fibre, and imparting to it a distinct colour; therefore, we do not obtain white grounds by dyeing white grounds with madder, logwood, nutgalls, yellow wood, quercitron, &c., but must have recourse to other means, as bleached grass, soap, bran and chloride of soda.

Relation of the cotton, combined with the colouring matter, to mordants. When calico is boiled with nutgalls, it acquires a yellowish colour, which, by being dipped in a solution of iron alum, is turned into a dark gray. When boiled a second time, in the nutgall solution, it becomes still darker. Bablah and madder produce similar results.

This mode of dyeing differs from the common method in this respect, that the dye is applied twice, and the mordant only once; viz., the cloth is first dyed, then mordanted, and then again dyed. In this way we obtain, without much trouble and loss of time, very equal grounds; and we have it in our power, by repeating the dyeing and mordanting, to produce a variety of shades.

The properties of cloth dyed with nutgalls and iron alum, are similar to those of nutgall gray. The different colours produced from these materials, by modifying their application, are remarkable. When the mordant is employed last, as in nutgall gray, a combination is formed with excess of mordant; if the proceeding is reversed, a combination is produced with excess of gallic acid. The former approaches blue, the latter red.

Gray, from bablah and iron alum. The cloth is dipped in a solution of iron alum (1 iron alum to 60 water), pressed and then washed. To make the colour clearer, the cloth may be previously boiled for a quarter of an hour, in an infusion of bablah. The dying is performed at the boiling temperature. To form the first colour, 10 lbs. of cloth are employed, with 1 lb. bablah. To produce the second, the quantity of bablah should be triple.

Tan acts in the same way as bablah, and gives similar shades. But what is very remarkable, oak bark, willow bark, and persian berries, employed in the same way, make no difference. Specimens dyed twice are not darker than those dyed once.

Violet from madder and iron alum.—The cloth is dipped in

a solution of iron alum (1 iron alum to 60 water), pressed and then washed. When it is dyed twice, it is mordanted in the same solution, and afterwards buried for half an hour in a solution of Avignon madder. The first colour, which is darker than the second, is produced by means of 12 lbs. cloth, and 8 lbs. Avignon madder; the second by 12 lbs. cloth, and 14 lbs. Avignon madder.

Properties of madder violet.—When passed through hot soap-suds, the colour becomes blueish. When boiled for ten minutes in the same mixture, it becomes, on the contrary, reddish and paler, without losing any of its clearness.

Solution of potash forms a violet spot, which is removed by rinsing in water.

Lime water, ammonia, and vinegar, have no action.

Lime juice forms yellow spots, which are removed by ammonia.

Both of the *tin mordants* discharge a chamois colour.

Solution of chloride of lime produces reddish white spots.

Remark. The iron alum facilitates the exhibition of such grounds, as are here noticed, in reference to its equable distribution over the calico. Other salts of iron, as sulphate of iron, might be employed, but we are not so sure that the cloth will be dyed free from spots.

ALUM MORDANTS.

Alum is employed as the mordants mentioned, for light colourless grounds. We dissolve 5 lbs. alum in 300 water, and impregnate the calico with it in the manner described, by dipping, pressing, and rinsing. There remains in this case, a considerable quantity of alum in combination with the fibre of the calico. Hence, the colour, though pale, is very equal. Oak bark, with alum, forms a pale yellow colour; catechu with alum, a brown orange.

Experiment shews that the same result follows, whether we employ strong or weak solutions of alum, as mordants. Calico only takes up a certain quantity of alum; so that a solution of alum, containing five times as much salt as that given above, does not produce darker colours. If carbonate of soda is added to the solution of alum, a mordant is ob-

tained, which gives darker colours than the common solution of alum. With this object in view, dissolve 32 lbs. alum in 80 lbs. boiling water, and add gradually a hot solution of 11lbs. crystallized carbonate of soda in 80 lbs. of water, and continue the heat as long as any white flocks remain ,in the solution, which will at last disappear with effervescence. This mordant cannot be printed, as it does not thicken with starch. It does not give dark colours, but only light grounds. Hence, the mordant is not fitted for superseding the acetate of alumina, as is stated in many works on dyeing.

When the calico is first boiled, with tan or nutgalls, and then impregnated with this mordant, the colours produced are a little darker.

Acetate of alumina mordant.—This mordant is formed by mixing the solution of alum with sugar of lead. The quantity of sugar of lead should always be equal to that of the alum, as it has been found that this proportion, by which much alum remains undecomposed, gives the clearest red and yellow colours.

Calico takes up a greater quantity of mordant from a solution of the acetate of alumina, than from an equally strong solution of alum. When the calico is rinsed, after remaining equally long in both solutions, and before being dried, the effect of the sugar of lead may be exhibited by applying two mordants, one consisting of 1 lb. alum, and 40 lbs. water; and the other of 1 lb. alum, 40 lbs. water, and 1 lb. sugar of lead; and dyeing the cloth in the same madder solution. The second is darker and clearer than the first.

The cause of this difference depends on the circumstance that the acetic acid does not combine so firmly with the alumina as the sulphuric acid; hence the calico, which here acts as an acid, can take up more. It is also possible, by means of acetate of alumina mordant, to fix still more alumina upon the calico, by allowing the cloth saturated with it to dry. By this means a portion gives out acetic acid (which does not occur with sulphuric acid, when alum alone is employed), and the calico is then so strongly mordanted, that it gives a very dark red with madder.

For the exhibition of the different dark colours, the dyer should keep the three following mordants, or at least a sufficient quantity of number 1, in order to mix the others. Alum mordant, No. 1, 300 lbs. alum, 800 lbs. water, 300 lbs. sugar of lead.—Alum mordant, No. 2, 300 lbs. alum, 1200 lbs. water, 300 lbs. sugar of lead.—Alum mordant, No. 3, 300 lbs. alum, 1600 lbs. water, 300 lbs. sugar of lead.

The preparation of these mordants requires the following precautions : The alum should be bruised, and placed in a wooden cask; then hot water should be digested on it, and the mixture stirred till the alum is dissolved. Then the sugar of lead may be added, the mixture stirred, and allowed to become clear. These mordants are not fit for use till after 24 hours.

As the pounding of the alum is somewhat troublesome, it may be dissolved in one half of the water at the boiling temperature. The copper vessel in which this is performed should, however, be protected by tin from the soluble power of the alum.

It is peculiarly advantageous, when employing madder red, to add carbonate of soda to the mordant.

For 300 lbs. alum we take 30 lbs. crystallized carbonate of soda, dissolved in 'water. Add the alum solution gradually to it, and then the sugar of lead. It would be an error to add the soda after the sugar of lead; in this case, alumina would be precipitated, which would not again dissolve.

All these mordants contain, by consequence, iron, when the alum contained iron. To make a mordant free from iron, the addition of prussiate of potash may be had recourse to in the proportion of 1 lb. prussiate of potash to 300 lbs. alum.

The prussiate of potash should first be dissolved in water, and the boiling solution of alum added.; the mixture being stirred, a dark blue colour will be produced, which disappears on the addition of the sugar of lead; the sulphate of lead taking up the Prussian blue, and falling to the bottom. It is, however, necessary to mix the solution of the prussiate of potash with the boiling solution of alum, and to allow them to remain for some time in contact, or allow

them to boil together, otherwise the sulphate of lead does not precipitate all the Prussian blue.

As the acetate of alumina becomes flocky by boiling, the sugar of lead should not be added to the boiling solution of alum; but it should first be allowed to cool, or what is still better, the alum may be dissolved in one half of the water to be employed, and the other half added in the cold state, when the prussiate of potash has finished its action. By this means the solution will be sufficiently cooled, and the sugar of lead can be added without injury to the solution.

RED FROM COCHINEAL AND ALUM MORDANT.

This colour is of little value, as it is not sufficiently clear. Its exhibition on the calico is also attended with many difficulties. The solution must be made with water, completely free from lime, otherwise a great part of the cochineal is subtracted, the colouring matter combining with the lime, and becoming insoluble.

Properties of cochineal red.—This colour is very unstable. It does not withstand the action of light and air. Boiled soap-suds, which contain 1 lb. of soap dissolved in 200 lbs. of water, make the colour half as dark, while they become dark red. It therefore does not stand washing with soap.

Potash forms brown spots, which are again restored by vinegar.

Ammonia makes the colour paler, and dissolves it.

Lime juice forms yellow spots, which are not completely removed by ammonia.

Vinegar does not alter the colour.

Solution of chloride of lime bleaches the red very quickly, and leaves a white pattern.

Tin mordants, No. 1, and 2, only remove the colour partially, and hence they cannot be employed to discharge it.

RED FROM FERNAMBUC AND ALUM MORDANT.

A light red is obtained by impregnating the cloth with the alum mordant, No. 3, by dipping, pressing, and rinsing. To form a dark red, alum mordant, No. 1, is used, by dipping it equally, and allowing the cotton to dry.

The Mordanted Cloth is purified by rinsing it in running water.

Dyeing.—To form a dark pattern for 12 lbs. of mordanted cloth, 4 lbs. of fernambuc and 4 lbs. of bran are requisite. The fernambuc and the bran should be boiled first with a little water; then more of the latter should be added, and the well moistened cloth placed in it. The dyeing should be completed at a boiling temperature; still so much dye remains in the solution that a fernambuc - red may be produced.

Clearing.—Soap-suds formed of 1 lb. oil-soap and 600 lbs. lukewarm water remove the yellowish colour of the alumina and change it into reddish blue. When, however, bran is employed in the dyeing, this clearing is quite unnecessary. Per-chloride of tin changes the colour into bright red when the cloth is passed through a cold solution of 1 lb. chloride of tin in 2000 lbs. water.

Properties of Fernambuc-red.—This colour is more permanent than cochineal-red; but still is very fleeting as it is readily decomposed by air and light. Boiling soap-suds formed of 1 lb. soap in 200 water render the colour considerably brighter, while the soap-suds take up dye, and become dark-red. It disappears in the washing.

Solution of potash forms clear purple spots which vinegar removes.

Ammonia and lime-water produce a reddish-blue shade.

Vinegar occasions no change.

Lime-juice makes reddish-yellow spots which are completely dissolved by ammonia.

Tin mordant, No. 1. makes purple-red spots.

Tin mordant, No. 2. makes bright-red spots.

Solution of chloride of lime consisting of 1 lb. chloride of lime in 40 lbs. water, when printed on the cloth produces in ¼ of an hour no remarkable effect; but after some further time, it is bleached, a property which distinguishes it from cochineal.

RED FROM MADDER AND ALUM MORDANT.

The mordant is applied in the same way as has already been described under bright-red. To form a dark madder-red, the alum mordant, No. 1. is applied by saturating the calico, drying it rapidly, and· then allowing it to hang for eight days in an airy place.

The mordanted calico is cleansed by rinsing it in running water.

As in no department of dyeing is there more detriment experienced than in madder-dyeing, by allowing any mordant to remain uucombined with the cloth, and to dissolve in the solution; it is proper to pass the cloth after rinsing through warm water.

Common madder-red, when it comes out of the boiler, has either a brown or a dirty brick-red colour, and it acquires, with great difficulty, the proper red shade. The latter it acquires, however, in a high degree by the addition of bran in dyeing. If we employ a greater quantity of bran than 3 lbs. bran to 1 lb. madder, we obtain brighter colours. The result is equally good whether we employ 9 lbs. mordanted calico, 12 lbs. madder, 36 lbs. bran, or 24 lbs. madder and 72 lbs. bran.

If the proportion of the bran to the madder is diminished, colours are produced which approach a brown shade.

The quantity of madder required to dye colourless grounds depends on the kind employed. Avignon and Dutch madder are almost equal in their powers of dyeing, while the red kinds are less so. By different trials, I have ascertained that 5 lbs. of Silesian summer-red madder does not produce darker colours than $2\frac{1}{2}$ lbs. of Avignon madder.

The varieties of madder raised by Herr Charles Milde, at Breslaw, with great care, are equal to the superior madders.

In general, it may be said, that equal weights of mordanted cloth and madder afford a sufficiently saturated red, although in many kinds a fourth more is required.

The *dyeing* of madder-colours, in order to be equable, requires a considerable time. When there is so much cloth as to require 2 or 3 hours, the temperature of the solution, during the first hour, should not exceed $99°\frac{1}{2}$ or $110°\frac{1}{4}$; from the first to the second hour, from $155°\frac{3}{4}$ to $167°$; from the second to the third hour, the solution may be brought to the boiling-point, and kept there for half an hour.

Clearing.—Madder-red dyed with the addition of bran, requires to be cleared by passing it through soap-suds con-

sisting of 1 lb. oil-soap, dissolved in 200 lbs. of boiling water. This gives the dye a pink tint, while the yellow disappears. A cold strong solution of carbonate of soda ($54\frac{1}{2}°$ to $66\frac{1}{4}°$) possesses a similar action.

A solution of salt of tin in caustic potash employed in a very dilute state, and cold, imparts more lustre to the madder red.

Properties of madder red.—This colour is very permanent when exposed to air and light, although not so durable as Turkey red.

By the common washing with soap, the dye acquires brightness; but is not much clearer if the solution employed is too hot.

Solution of potash makes the colour, when heated with soap-suds, only a little darker. The colour is restored by vinegar.

Ammonia produces no change.

Lime water makes a slight alteration, which disappears by washing.

Vinegar and Lime Juice produce no change.

Tin mordant, No. 1 and 2, printed in a strong solution, occasion no loss of colour.

Solution of chloride of Lime acts like the *tin mordants.* When the dyed cloth is allowed to remain for two hours, in a solution of 1 lb. chloride of lime in 40 lbs. of water, a loss of colour, but not a complete bleaching is experienced.

YELLOW FROM PERSIAN BERRIES AND ALUM MORDANT.[*]

The *mordant* employed is alum mordant, No. 3: the cloth is soaked in it, pressed and washed. The solution of alum with soda may also be employed for mordanting in the same manner.

Dark berry yellow is produced by alum mordant, No. 2; allowing the cloth to be soaked in it, pressed and then dried. The mordanted cloth is cleared by rinsing it in running water. Bran has an advantageous action in the production of this colour, which becomes purer and clearer.

[*] See Specimen, "Records of General Science," i. 325.—EDIT.

The proportions are 8 lbs. cloth, 1 lb. Persian berries, and 3 lbs. bran.

Dyeing.—In this there is no great difficulty; as the dye does not readily become parti-coloured. The berries and the bran are first boiled with a little water, and much cold water is then to be added, and the well-moistened cloth placed in the solution. The solution can always be heated to the point of ebullition. The presence of the bran prevents any injurious effects from a boiling temperature.

Clearing. When the cloth is passed through warm soap-suds, consisting of 200 lbs. water and 1 lb. soap, the cotton acquires more lustre and more permanence.

Tin mordant, No. 1, gives the dye more orange, when the cloth is placed in a mixture of 1 lb. tin mordant, No. 1, and 1000 lbs. water.

Properties of berry yellow. This colour is one of the most permanent yellow colours known. It withstands the action of light and air for a considerable time.

Boiling soap-suds extract from the cloth very little of the dye, while the water is strongly coloured yellow.

Solution of potash makes brownish yellow spots, which vinegar converts into greenish yellow.

Lime water acts like potash.

Ammonia does not change the colour, but dissolves some of it, and becomes yellow.

Vinegar produces no action.

Lime juice forms spots like dark quercitron yellow, which ammonia completely restores.

Tin mordant, No. 1, colours those places on which it is printed, orange yellow.

Tin mordant, No. 2, does the same, only in a weaker degree.

Solution of chloride of lime (1 chloride to 40 water) does not destroy the colour, but renders it brown. When a piece of cloth is allowed to remain a quarter of an hour in the solution, the brown colour becomes brighter, until the shade attains that of a clear yellow rust colour.

(*To be continued.*)

Notice of some Recent Improvements in Science.

ELECTRICITY.

Electricity by contact.—According to Karsten, the metals, and, perhaps, all solid bodies, become positive in fluids; and the fluid in which they are plunged becomes negative.

2. A solid, which is half immersed in the fluid, acquires an electric polarity; the submersed portion possesses positive electricity, and that which is not immersed, negative electricity.

3. Solid bodies present a great difference in their electro-motive force, in relation to the same fluid, and this difference is the true cause of the electrical, chemical and magnetic activity of the circuit.

4. If two solid electromotors, but of different electromotive force, are immersed in the same fluid without touching, the most feeble electromotor receives the opposite electricity to that of the strongest electromotor, and becomes, of consequence, negatively electric.

5. The half of the weakest electromoter, which rises above the fluid, exhibits the opposite electricity to that of the immersed portion, and becomes, consequently, positively electrical.

6. The electro-motive electricity of a fluid depends on the property of being reduced by two solid electromotors, of different force, to such a state that the two electromotors receive opposite electricities. In general, all fluids which are bad conductors of electricity, possess the property which has been pointed out; but not fluids which are not conductors, nor those which are good conductors. However, the intensity of the electro-motive force of fluids, does not depend only on the more or less imperfect conductibility, but on other relations which are not sufficiently known.

7. The electro-motive effects of two metals, which form a circuit in the same fluid, are founded upon the continual excitation and neutralization of the opposite electricities which take place in the fluid. They are produced by the

electro-motive action of the strongest and weakest of the electromotors upon the fluid, and are accelerated by the immediate contact of two solid electromotors, when the latter are good conductors.

8. The chemical changes which take place in the fluid are, it is true, in proportion to the neutralization of the two electricities produced by the solid elements of the chain. But these chemical changes, and the neutralization, do not follow as cause and effect.

9. In the system of chains which forms the pile of Volta, the opposite electricities are completely neutralized by the solid elements of each chain ; that is to say, by the pairs of plates ; and there is no electrical current from one pair to the other. (*L'Institut.* 150.)

Electricity from deoxidation. It is well known, that when the peroxide of manganese is brought in contact with platinum, positive electricity passes into the platinum, and the negative into the finger, or whatever body the peroxide is touched with. De Larive has ascertained that the developement of the electricity proceeds from chemical action, as it is very weak with distilled water, but becomes stronger with acids or alkaline solutions; for wood being substituted for the platinum, the same effects are produced when the finger is dipped in an acid or alkaline solution, and applied to it. (*Ibid*, 155.)

Peroxide of Lead, according to Munck, when brought in contact with other electromotors, as copper, zinc, carbon, and peroxide of manganese, developes negative electricity much more strongly than any other body hitherto examined ; and forms an excellent conductor of electricity. Hence, it may be employed with great advantage in the construction of dry piles, and even in common piles, instead of copper. (*Poggendorff's Ann.* 1835-6.)

HEAT.

Temperature of Vapour. It is generally supposed that the vapour of water disengaged from a boiling saline solution, has exactly the same temperature as the upper layer of this solution ; and that the vapour possesses only in this case an elasticity equivalent to the pressure of the atmo-

sphere, and, consequently, less than it would have at its
maximum density, at a temperature much above 212°.   It
appears natural to suppose, that each bubble of vapour which
is elevated, immediately acquires the temperature of the li-
quid, which surrounds it on all sides, and continues at the
same time, to undergo a certain expansion, until its elasticity
is equal to the pressure of the atmosphere.   Rudberg,
however, has drawn a different conclusion from his expe-
riments.   (*Poggendorff's Ann.* 1835, *No.* 2.) He has found
that the temperature of the vapour produced from a boil-
ing saline solution is independent of the nature and qua-
lity of the salt dissolved; it is under the same barometric
pressure, absolutely the same as the vapour raised from
pure water.   He also concludes that the temperature of the
vapour which rises from a boiling saline solution, is, at
whatever pressure the ebullition takes place, always the
same, in the same circumstances, as that of the vapour of
boiling water.

These results are quite different from those obtained by
Dalton, Gay Lussac, and Princep, with regard to the
vapour formed from saline solutions, by evaporation. They
consider that the vapour of a saline solution has much less
elasticity than that of pure water, if the two liquids are at
the same temperature.   Inversely, then, it follows, that
for the same elasticity, the vapour of a saline solution is
hotter than that of pure water.   They find, also, that this
difference of temperature increases both the quantity of
salt dissolved, and that it varies much, according to the
nature of the salt.   Hence, it appears, that between the
temperature of vapour and its elasticity, the relation is
quite different, if this vapour is produced by the ebullition
of a saline solution, or by evaporation from the surface.
The cause of this difference requires further investigation.

Chemical action of the solar spectrum.   Professor Hessler,
of Grätz, has observed, that the action of the solar spec-
trum, upon a piece of paper, covered with gum water and
chloride of silver, varies according to the nature of the
prism employed.   The time required with water and spirit
of wine, was very short; with turpentine and oil of cassia,
two to thirteen minutes: with flint glass, 2′ 3; with crown
glass, 1′5.   The maximum lay in the spectrum of the spi-

rit of wine, in the violet near the blue; with the water in the middle of the violet; with the oil of cassia 23 lines beyond the violet border.—(*Baumgartner's Zeitschrift.* iii. 336.

Dilatation of absolute alcohol and sulphuret of carbon, by heat. Professor Muncke, of Heidelberg, has made experiments, during several ,winters, to determine this point accurately. He also studied the dilatations of pure water, artificial sea water, sulphuric ether, petroleum, ammonia, muriatic acid, nitric acid, sulphuric acid, and almond oil; he found their maximum density to exist at the following temperatures:

Pure water . . . . 38·80 F.
Artificial sea water . . . . . 22·55
Alcohol not pure (sp. g. 808 at 54½°)—69·8, or—56·6 C.
Sulphuric ether (sp. g. 733 at 54½°)—32·8
Petroleum (sp. g. 78·125 at 54½°) —96·7
Absolute alcohol (sp. g. 791 at 32°)—129·1

Muncke found that he could heat sulphuric ether in a straight thermometer tube up to 107·6°, without boiling, and once as high as 122°; rectified petroleum he raised to 212°, without boiling it; and sulphuret of carbon to 149°; although the common boiling temperature of these liquids are 95°, 185°, and 116°. He attributes this phenomenon, which has been previously observed, to the adhesion of the liquid to the sides of the tubes, and to the difficulty of evaporation. Muncke concludes, from his experiments, that petroleum is much better adapted for thermometers than alcohol; but that sulphuret of carbon is still better. Petroleum must congeal below —95°, and sulphuret of carbon still lower; neither of them has been solidified. The sulphuret, when well made, is always of the same quality. The order of their dilatations at 122° is, sulphuret, 60723, Petroleum, 56071, Alcohol, 52632.*

Temperature of carbonic acid disengaged from different sources.—Bischof disengaged carbonic acid from carbonate of lime, by exposing it to a strong heat in a cannon. A thermometer placed in the current of gas, at the muzzle, rose up to 88¼°, and remained there, whatever intensity was given to the heat. In another experiment, he disen-

* Memoires de St. Petersbourg, ii. 1835—36.

gaged the carbouic acid by means of sulphuric acid, in a matrass, supplied with a tube. A thermometer was placed at the end of the tube. The temperature of the chalk, sulphuric acid, and air, before the experiment, was from 53° to 54½°. When the sulphuric acid was poured on the chalk, the thermometer rose to 86°. Bischof was induced to make these experiments, for the purpose of determining certain points connected with the state of the earth. Some philosophers attribute the heat of acidulated mineral waters to the carbonic acid, which these waters absorb at a great depth; and consider that the developement of this gas is the result of the decomposition of masses of limestone, produced by a strong heat in the centre of the earth. These experiments, however, show, that the greater part of this heat, which the decomposition of the carbonate of lime requires, is absorbed by the carbonate, to form carbonic acid gas.—(*Poggendorff's Ann. No.* 5, 1835.)

Temperature of thermal waters.—Some recent observations had tended to shew, that the temperature of hot springs might vary. Legrand has found that, in reference to the springs of the Eastern Pyrenees, observations of 80 years correspond with those of recent date. In 1754, Carera found the temperature of the basin which supplies the bath at Arles, 156·8, which, when corrected by the tables of Deluc, is 142¼°. Now, this is precisely what M. Anglada found it in 1820. M. Lemonnier has obtained the same results, with regard to the hot springs of Mont-Doré. —(*L'Institut.* 150.)

CAPILLARY ATTRACTION.

1. Dutrochet, some years ago observed, that when two distinct fluids in a tube, are separated from each other by a partition having capillary pores, the liquid soon begin to pass in currents through the dividing medium; but the quantity of liquid in each is not the same; so that one of the fluids acquires a greater volume than the other. The stronger stream Dutrochet terms *endosmose*, and the weaker current *exosmose*. Some have supposed, that the difference in the adhesion of the particles of different liquors, was the cause of this phenomenon; and that the *endosmose* always took place from the side of the less glutinous fluid. But when

solutions of gum and sugar were tried, the endosmose took place from the gum to the sugar, even when the former was twice as much concentrated as the latter. Many acids, as nitric, muriatic, phosphoric, and acetic, when they are separated from water, by an animal membrane, receive the endosmose from the latter. Concentrated sulphuric acid destroys the membrane; and when diluted, exhibits no signs of endosmose. When oxalic acid and water are employed, the endosmose proceeds from the acid to the water, and increases in proportion to the strength of the solution. By itself, however, oxalic acid passes more slowly through animal membrane than water. When a solution of tartaric acid exceeds the specific of 1·05, the endosmose takes place from the water to the acid; if it is lighter than 1·05, the process is reversed. The same happens with citric acid.

Dutrochet terms the passage of the oxalic acid to the water, inverse endosmose. The mineral acids do not exhibit this phenomenon; phosphoric acid, however, exhibits it for an instant, when reduced by the addition of water, to the specific gravity of 1·085. Change of temperature affects the passage of the acid through the membrane, as it does its specific gravity.

This agrees with the experiments of Girard, upon the flowing of pure water, and water containing Nitrate of potash, through capillary glass tubes. A solution of one part nitrate of potash in three parts water, at the temperature of 40°, flows more readily through a capillary tube than pure water; while above 40°, the reverse happens. Dutrochet has found these observations to hold only with animal membranes; not with vegetable, or thin inorganic porous plates.—(*Pharmaceutisches Central-blatt.*, *Feb.* 1836, 92.)

2. Jerichau,[*] of Copenhagen, has obtained some interesting results on this subject. A forked glass tube, 1½ line in diameter, was closed at one extremity with sealing wax, and then the closed leg was filled with water, the covered portion with mercury, and the open leg partly with an aqueous solution of sugar. The tube was placed in a vertical position.

[*] Poggendorff's Annalen, xxxiv. 613.

In the course of a week the mercury in the closed leg rose a line, which might lead to the supposition that the wax had not been so closely applied to the glass as to prevent the existence of any capillary opening between the wax and glass. To determine this point another equally curved tube was taken, fused at one extremity, and filled as before. To ascertain the smallest rise, a small mirror, with a transverse scratch upon it, was placed between the legs of the tube, close to the bent leg; so that the scratch, when it cut the reflexion of the eye over the pupil, appeared as a tangent to the upper surface of the mercury in the tube.

On a small rise of the mercury, the eye must be brought forward, when the scratch appears still to be the tangent of the mercury, and passes immediately through the middle of the reflexion of the eye.

After trial, it was found that a tube fused at one end, and about a line in diameter, answered the desired purpose. This tube was filled with water, and then a globule of mercury, which occupied 0·7 line of the tube, was placed in it; some pulverized sugar was dissolved in the water. The tube was then made fast to the mirror, and placed horizontally, in order that the mercury might not be pressed down by its own weight. The drop slowly progressed towards the closed end of the tube. After the lapse of a month, it had advanced about a line. The water which was expelled, mixed itself with the sugar solution; and this was concentrated by evaporation, without depositing any crystals. A solution of gum, substituted for that of the sugar, gave the same result. In ten days, the drop of mercury advanced 0·2 line towards the closed end of the tube.

The converse of this experiment was tried with a portion of a straight tube, which was fused at one extremity. A dense solution of sugar was placed between the closed end and a drop of mercury; and before the drop some water was introduced. If the latter evaporated, it was renewed. The drop advanced towards the open end of the tube, while the saccharine solution increased in volume by the absorption of water.

Extending his researches in this way, with different liquids, Jerichau draws six inferences from his experiments.

1. That liquids, separated by porous plates, reciprocally penetrate in part through these plates. This, however, has been previously stated.

2. The proportion of the volume, which passes from both solutions in equal time, depends on the nature of the solutions and partition as well as the temperature. It is not, however, a necessary condition, that a greater volume should pass through the plate from the one solution than from the other; or, that on one side of these plates a greater volume should enter, as Dutrochet erroneously thinks.

3. When the diffusion is terminated, the volume remaining on each side of the partition may be calculated from the original volume, being inversely, as the square-root of its density; as Graham has shewn with regard to gases. If equal volumes of a saturated solution of common salt, and solution of sugar of the specific gravity 1·078, are separated by a bladder, the first increases in volume at first, but diminishes in specific gravity, in a greater degree than would take place by a regular mixture.

4. The height to which solutions ascend in capillary tubes, is often proportionate to the quantity of liquid diffused. Thus, some solutions, which rise highest in capillary tubes, are conveyed in strongest streams, but there are many exceptions to this rule.

5. Liquids are not only conveyed through solid porous plates, but also through a short canal between mercury and glass.

6. By the chemical action of electricity, the proportion of the liquid which passes may be increased; but this cause only operates, in so far as it separates acids, alkalies, and salts.

ARTICLE X.

ANALYSES OF BOOKS.

On the Application of the Hot Blast, in the Manufacture of Cast Iron. By Thomas Clark, M.D., &c. (Trans. Royal Soc. Edinb. xiii.)

THE substitution of hot for cold air, in the blast furnaces of the iron manufactory, is an improvement which suggested itself to the inge-

nious Mr. Neilson, of Glasgow, at a most seasonable period; when the great demand for iron in the construction of railways is daily, nay, hourly, increasing. It is remarkable, that in this country, the practice is almost universally adopted; but hitherto it has been scarcely noticed in English books. The French and Americans, on the contrary, have collected and published a great mass of information, chiefly, of course, derived from practical sources in this country. Dr. Clark has supplied the void which we have noticed, concisely—but distinctly.

His paper investigates the subject under three heads. 1st. The process of making iron as formerly practised. 2nd. Mr. Neilson's alteration in that process. 3rd. The effect of that alteration. And, 4th. The cause of that effect.

1. The original process consisted in introducing a charge of coke, limestone, and *mine,* or burned ironstone, into the top of the iron furnace; and this mixture was excited to combustion by air forcibly driven in, at about forty feet from the top, through pipes from a blowing apparatus. The iron was thus separated from carbonic acid, alumina, and silica; and was allowed to run off at the bottom.

2. Mr. Neilson improved this process, by substituting for air at the temperature of the atmosphere, air heated up to 300° and upwards. This is effected by passing the air through the cast-iron pipes, through which the former passed, kept at a red heat.

3. During the first six months of the year 1829, when all the cast-iron in Clyde iron-works was made by means of the cold blast : a single ton of cast-iron required for fuel to reduce it, 8 tons 1¼ cwt. of coal converted into coke. During the first six months of the following year, while the air was heated to near 300° Fahr.: one ton of cast-iron required 5 tons 3¼ cwt. of coal, converted into coke.

The saving amounts to 2 tons 18 cwt. on the making of one ton of cast-iron; but from that saving comes to be deducted the coals used in heating the air, which were nearly eight cwt. The nett saving thus was 2½ tons of coal on a single ton of cast-iron. But during that year, 1830, the air was heated no higher than 300° Fahr. The great success, however, of those trials, encouraged Mr. Dunlop, and other iron masters, to try the effect of a still higher temperature. Nor were their expectations disappointed. The saving of coal was greatly increased, insomuch that, about the beginning of 1831, Mr. Dixon, proprietor of Calder iron-works, felt himself encouraged to attempt the substitution of raw coal for the coke before in use. Proceeding on the ascertained advantages of the hot blast, the attempt was entirely successful: and since that period, the use of raw coal has extended so far as to be adopted in the majority of the Scotch iron-works. The temperature of the air under blast had now been raised so as to melt lead, and sometimes zinc, and therefore was above 600° Fahr., instead of being 300°, as in the year 1830.

" During the first six months of the year 1833, when all these changes had been fully brought into operation, one ton of cast-iron was made by means of 2 tons 5¼ cwt. of coal, which had not pre-

viously to be converted into coke. Adding to this eight cwt. of coal for heating, we have 2 tons 13¼ cwt. of coal required to make a ton of iron; whereas, in 1829, when the cold blast was in operation, 8 tons 1¼ cwt. of coal had to be used. This being almost exactly three times as much, we have, from the change of the cold blast to the hot, combined with the use of coal instead of coke, *three times as much iron made from any given weight of splint coal.*

" During the three successive periods that have been specified, the same blowing apparatus was in use; and not the least remarkable effect of Mr. Neilson's invention, has been the increased efficacy of a given quantity of air in the production of iron. The furnaces at Clyde iron-works, which were at first three, have been increased to four; and, the blast machinery being still the same, the following were the successive weekly products of iron during the periods already named, and the successive weekly consumpt of fuel put into the furnace, apart from what was used in heating the blast:

	Tons.		Tons.		Tons.	
In 1829, from 3 furnaces,	111	Iron from	403	Coke, from	888	Coal.
In 1830, from 3 furnaces,	162	Iron from	376	Coke, from	836	Coal.
In 1833, from 4 furnaces,	245	Iron		from	554	Coal.

" Comparing the product of 1829 with the product of 1833, it will be observed that the blast, in consequence of being heated, has reduced more than double the quantity of iron. The fuel consumed in these two periods we cannot compare, since, in the former, coke was burned, and in the latter coal. But on comparing the consumpt of coke in the years 1829 and 1830, we find that although the product of iron in the latter period was increased, yet the consumpt of coke was rather diminished. Hence the increased efficacy of the blast appears to be expected, from the diminished fuel that had become necessary to smelt a given quantity of iron."

The temperature was so high, that it was found necessary, in order to prevent the melting of the cast-iron lining near the nozzles of the blowpipes, to substitute for the solid lining a hollow one, filled with water, which is continually changing, as it becomes heated.

4. Dr. Clark gives what we conceive to be the obvious explanation of the mode in which the hot air acts — Berthier, it is true, has broached another.—See " Records," ii. 151.) But it is far-fetched, and superseded by the more simple explanation presented by our author.—He observes,

" As nearly as may be, a furnace, as wrought at Clyde Iron-works in 1833, had two tons of solid materials an hour put in at the top, and this supply of two tons an hour was continued for 23 hours a-day, one half-hour every morning, and another every evening, being consumed in letting off the iron made. But the gaseous material, the hot air—what might be the weight of it? This can easily be ascertained thus: I find, by comparing the quantities of air consumed at Clyde iron-works, and at Calder iron-works, that one furnace requires of hot air from 2500 to 3000 cubical feet in a minute. I shall here assume 2867 cubical feet to be the quantity; a number that I adopt for the sake of simplicity, inasmuch as, calculated

at an avoirdupois ounce and a quarter, which is the weight of a cubical foot of air at 50° Fahrenheit, these feet correspond precisely with 2 cwt. of air a minute, or *six tons an hour.* Two tons of solid material an hour, put in at the top of the furnace, can scarce hurtfully affect the temperature of the furnace, at least in the hottest part of it, which must be far down, and where the iron, besides being reduced to the state of metal, is melted, and the slag too produced. ' When the fuel put in at the top is coal, I have no doubt that, before it comes to this far-down part of the furnace, the place of its useful activity, the coal has been entirely coked; so that, in regard to the fuel, the new process differs from the old much more in appearance than in essence and reality. But if two tons of solid material an hour, put in at the top, are not likely to affect the temperature of the hottest part of the furnace, can we say the same of six tons of air an hour, forced in at the bottom near that hottest part ? The air supplied is intended, no doubt, and answers to support the combustion; but this beneficial effect is, in the case of the cold blast, incidentally counteracted by the cooling power of six tons of air an hour, or two cwt. a minute, which, when forced in at the ordinary temperature of the air, cannot be conceived otherwise than as a prodigious refrigeratory passing through the hottest part of the furnace, and repressing its temperature. The expedient of previously heating the blast, obviously removes this refrigeratory, leaving the air to act in promoting combustion, without robbing the combustion of any portion of the heat it produces."

From a table appended to this paper, and furnished by Colin Dunlop, Esq., it appears that in 1829, the average weekly product of the Clyde iron-works was 110 tons, 14 cwt. 2 qrs., and the average of coals used to 1 ton of cast-iron was 8 tons, 1 cwt. 1 qr. with the cold air; while in 1830, these numbers were respectively, 162 tons, 2 cwt., 1 qrs., and 5 tons, 5 cwt. 1 qr. with heated air; and in 1833, 245 tons, and 2 tons, 5 cwt. 1 qr., also with heated air. The following table gives the materials constituting the charge in the several years :

Materials constituting a Charge :

		cwt.	qrs.	lb.
1829,	Coke - -	5	0	0
	Roasted Ironstone,	3	1	14
	Limestone, -	0	3	16
1830,	Coke, - -	5	0	0
	Roasted Ironstone,	5	0	0
	Limestone, -	1	1	16
1833,	Coal, - -	5	0	0
	Roasted Ironstone,	5	0	0
	Limestone, -	1	0	0

II.—*Travels in the United States of America, Canada, &c.* By
J. Finch, Esq. London: Longman & Co.

This work derives no small degree of interest from the circum-

stance of its author being the grandson of the great Priestley, whose splendid career was spent in doing good; and whose efforts were so disgracefully marred by his own countrymen—whose treatment of him is an eternal stain on the land. After visiting various parts of the United States and Canada, Mr. Finch bent his way to North-umberland, on the Susquehanna. He arrived at this town at noon, and inquired for the mansion and tomb of Dr. Priestley. The following remarks are interesting:

" In his youth he had to struggle with many difficulties.

" Fortiaque adversis opponite pectora rebus."

When thirty years of age he was minister of a small country church, with a salary of twenty-five pounds sterling a-year; a hesitation in his speech, which prevented his being a popular preacher, and his sentiments of religious truth were opposed. He had to contend with disease, poverty, and persecution.

" What had he to support him in this forlorn and desolate situation? His dependence was upon God, whom his enemies said he contemned; and that love of science which often renders its votary superior to evils which would crush other men. He was content if he could procure a few tests for his chemical experiments, or glass for an electrical machine. His first experiments were made with an apparatus that cost a few shillings, and by its means the world was made acquainted with the constituent parts of the atmosphere.

" Fortune began to be tired of persecuting a man who felt not her frowns, and his advancing age saw him gradually emerge from the clouds which seemed to envelop him.

" His finances became more favourable, and he finally enjoyed affluence.

" He was chosen a member of the most distinguished learned societies of the age.

" He lived to see his religious opinions adopted by numerous churches.

" He acquired honourable fame.

" He enjoyed the truest happiness that human life can afford— the society of those elevated by talent and virtue to a high station in society. He was the intimate friend of Lindsey, Barbauld, and Aiken; of Price, Watt, and Keir; of Shelburne, Galton, and Franklin; of Cavendish, Lavoisier, and Jefferson.

" The friend of those individuals must have been a happy and a distinguished man.

" He corresponded with the scientific men of the century in which he lived.

" I went to view his mansion, where the last few years of his life were passed. On the peaceful shore of the gentle Susquehanna he might congratulate himself,

" Di avere finalmente trovato un porto alla sua agitata fortuna."

The garden, orchard, and lawn, extend to the side of the river. A sun-dial, which still retains its station, was presented to Dr. Priestley by an eminent mathematician in London. Two large

willow trees grow near the mansion; under their shade he often
enjoyed the summer evening breeze.

" His laboratory is now converted into a house for garden-tools !
the furnaces pulled down! the shelves unoccupied !—the floor co-
vered with Indian corn !   A stranger might be inclined to say,

   " Sic transit gloria philosophiæ."

" But, when the chemist, or the historian, or the philosopher,
or the divine, examine the records of the various branches of
learning in which they are skilled, then will his name be honoured.
To this laboratory the children from the school were accustomed
to come, once a week, and he would amuse them with experi-
ments.

" The tomb of my grandfather, Dr. Priestley, is in the environs
of the town, surrounded by a low wall.   I knelt by my ancestor's
tomb, and the perils of my pilgrimage were remembered with plea-
sure."

ARTICLE XI.

SCIENTIFIC INTELLIGENCE, &C.

I.—*Plumbago and Black Lead Pencils.*

THERE is only one purpose to which this form of carbon, is applied
in the solid state, viz., for the manufacture of black lead pencils,
and its adaptation to this end depends upon its softness.   In the
state of a powder, plumbago is used to relieve friction.   Its power
in this way may be illustrated by rubbing a button first on a plain
board, five or six times, and applying it to a bit of phosphorus, the
latter will immediately burn.   When rubbed on a surface covered
with plumbago, double or triple the friction will be required to
produce the same effect.   One of the most remarkable circumstances
connected with plumbago is the mode in which it is sold. Once a year
the mine at Borrowdale is opened, and a sufficient quantity of plum-
bago is extracted, to supply the market during the ensuing year.   It is
then closed up, and the product is carried in small fragments of
about three or four inches long, to London, where it is exposed to
sale, at the black lead market, which is held on the first Monday
of every month, at a public-house in Essex-street, Strand.   The buy-
ers, who amount to about seven or eight, examine every piece with a
sharp instrument, to ascertain its hardness—those which are too soft
being rejected.   The individual who has the first choice pays 45*s.*
per pound; the others 30*s.*   But as there is no addition made to
the first quantity in the market, during the course of the year, the
residual portions are examined over and over again, until they are
exhausted.   The annual amount of sale is about £3000.   There
are three kinds of pencils, common, ever-pointed, and plummets.—
The latter are composed of one-third sulphuret of antimony and
two-thirds plumbago.

The 1st part of the process is sawing out the cedar into long planks, and then into what are technically termed tops and bottoms. The 2d, sawing out the grooves by means of a fly-wheel. The 3rd, scraping the lead on a stone; having been previously made into thin slices, to suit the groove; introducing it into the groove, and scratching the side with a sharp pointed instrument, so as to break it off exactly above the groove. The 4th, glueing the tops and bottoms together, and turning the cedar cases in a gauge.

The *ever-pointed* pencils are first cut into thin slabs, then into square pieces, by means of a steel guage. They are then passed through three small holes, armed with rubies, which last about three or four days. Steel does not last above as many hours. Six of these ever-pointed pencils may be had for 2s. 6d. If they are cheaper than this, we may be sure that they are adulterated.

In Paris, when you buy a sheet of paper in a stationer's shop, some of these pencils are added to the purchase. Now these are formed of a mixture of plumbago, fuller's earth, and vermicelli. Genuine cedar pencils must cost 6d. each. If they are sold at a lower price, they must be formed from a mixture, not from pure plumbago. Pencils are, however, sold as low as 4½d. a dozen.

There is no patent which has been more infringed on than that of Mordan's, for ever-pointed pencils. Birmingham is the source of this infringement, where they are sold as low as ¾d. each, formed of composition. A thousand persons are now engaged in the manufacture of these pencils, and pencil-cases.

These facts were stated by Dr. Faraday, at the Royal Institution, April 22nd.

II.—*Breakwater at Madras.*

Madras has been long well known as one of the most dangerous ports for shipping in the East. It is with no small degree of pleasure that we observe its enterprising inhabitants coming forward with liberal subscriptions, for the purpose of raising a breakwater. —*Madras Gazette, June,* 1835.

It is proposed that the work should be unconnected with the shore; that its shape should be a straight line parallel with the coast; that it should be of rough stone; and that it should be neither within, nor much beyond, the outer line of the surf. Its distance from the shore will be about 350 yards, and its length about 200. The depth of water is about 20 feet; and as it is intended to raise it five feet above high water, its total height will be 25 feet. The materials are to be brought down the Adyar, passed over the bar in trucks, or by means of cranes; and conveyed by sea on catamarans, to the site of the work. The estimated expense of the carriage is one rupee per cubic yard, and of the quarrying 1¾ rupee; and the whole expense as follows: Quarrying, 20,400 cubic yards, at 1¾ rupees, 35,700, conveying at one rupee = 20,400, total 61,100 rupees. But as government have liberally offered to furnish prisoners, &c. to assist in the work, it is hoped that the

whole of the expense of the quarrying will be saved to subscribers, leaving only 25,400 rupees to be provided. The subscriptions already amount to 40,000 rupees; leaving a large sum to cover unforeseen expenses. The whole work, it is proposed, should be similar in structure, to the Plymouth Breakwater; but, of course, it will be on a much smaller scale; that of Plymouth being in 56 feet water.

III.—*Meteors seen periodically on the* 12*th and* 13*th November.*

In 1833, professor Olmstead, of Newhaven, observed some remarkable meteors on the 12th and 13th of November,* and in 1834 he noticed a recurrence of these on the same days. He has brought forward sufficient proofs to substantiate the fact of their having been seen at a number of different localities. Professor Bache has, on the other hand, published the evidence of several persons, who did not see them. It appears from some accounts which we have received from the Cape of Good Hope, that Sir John Herschel observed a display of similar phenomena on the same day in 1835.— These facts are too remarkable to be overlooked, and may ultimately lead to some interesting deductions.

IV.—*The Existence of a New Planet suspected.*

On the 15th of February, M. Arago read to the Academy of Sciences, an extract of a letter from M. Cacciatore, Astronomer at Palermo, to Captain Smyth. The Sicilian astronomer announces in this letter that he saw, in the month of May, 1835, near the 17th star of the 12th hour of the catalogue of Piazzi, another star of the 7th or 8th magnitude. Having taken the distance of the two stars, he found that in three days the distance had increased. The motion of the second star was about 10 seconds of right ascension on the eastern side, and a minute, or a little less, towards the north. In consequence of the state of the weather, he could not succeed in tracing it. From the slowness of its motion, he conceives it must be situated beyond Herschel.—*(Bibliotheque Univ. Jan.* 1836.

V.—*Hypothesis respecting the Greater Quantity of Rain which falls at the Surface of the Earth, than at considerable Elevations.*

When we find two individuals, at distant periods, coinciding in their deductions from observation, without having any knowledge of each other's views, we cannot fail to infer that the latter bear with them the stamp of correctness, or at least of plausibility,— whatever may be thought of the want of research exhibited by the second person. Such a remark is applicable to the theological

* Silliman's American Journal, for 1835.

theory of Lyell, which is just that of the sagacious Ray; and to the explanation presented by Professor Phillips, of King's College, London, as to the remarkable fact, long known, that a rain guage, placed at the bottom of a building, always collects more rain than one placed at the top of the edifice. The inference drawn by him was, " that the whole difference in the quantity of rain, at differ-ent heights above the surface of the neighbouring ground, is caused by the continued augmentation of each drop of rain from the com-mencement to the end of its descent."—(*Third Report of the Bri-tish Association, p.* 410.)

We are indebted to Professor Bache, of Pennsylvania Univer-sity—(*Journal of the Franklin Institute, Feb.* 1836, 106), for the information that the same hypothesis was suggested in 1771, by Dr. Franklin, in a letter to Dr. Percival, published in the *Man-chester Memoirs* for 1784. Dr. Franklin observes, " I suppose it will be generally allowed, on a little consideration of the subject, that scarce any drop of water was, when it began to fall from the clouds, of a magnitude equal to that it has acquired when it arrives at the earth ; the same of the several pieces of hail; because they are often so large, and so weighty, that we cannot conceive a possi-bility of their being suspended in the air, and remaining at rest there for any time, how small soever; nor do we conceive any means of forming them so large, before they set out to fall. It seems, then, that each beginning drop and particle of hail, receives continued additions in its progress downwards. This may be in several ways ; by the union of numbers in their course, so that what was at first only descending mist, becomes a shower; or by each particle, in its descent through air that contains a great quantity of dissolved wa-ter, striking against, attaching to itself, and carrying down with it, such particles of that dissolved water as happen to be in its way; or attracting to it such as do not lie directly in its course, by its different state, with regard either to common or electric fire, or by all these causes united.

" In the first case, by the uniting of numbers, larger drops might be made; but the quantity falling in the same place would be the same at all heights; unless, as you mention, the whole should be contracted in falling, the lines described by all the drops converg-ing, so that, what set out to fall, from a cloud of many thousand acres, should reach the earth, in perhaps a third of that extent, of which I somewhat doubt. In the other cases we have two experi-ments.

" 1. A dry glass bottle, filled with very cold water, in a warm day, will presently collect, from the seemingly dry air that sur-rounds it, a quantity of water that shall cover its surface, and run down its sides, which, perhaps, is done by the power wherewith the cold water attracts the fluid, common fire, that had been united with the dissolved water, in the air, and drawing the fire through the glass unto itself, leaves the water on the outside.

" 2. An electrified body, left in a room for some time, will be more covered with dust than other bodies in the same room, not

electrified, which dust seems to be attracted from the circumambient air.

" Now we know that the rain, even in our hottest days, comes from a very cold region. Its falling sometimes in the form of ice shews this clearly ; and, perhaps, even the rain is snow or ice when it first moves downwards, though thawed in falling; and we know that the drops of rain are often electrified ; but these causes of addition to each drop of water, or piece of hail, one would think, could not long continue to produce the same effect : hence the air through which the drops fall, must soon be stripped of its previously dissolved water, so as to be no longer capable of augmenting them. Indeed, very heavy showers of either, are never of long continuance, but moderate rains often continue so long as to puzzle this hypothesis. So that, upon the whole, I think as I intimated before, that we are yet hardly ripe for making one."

Notwithstanding this cautious remark of Franklin, it is obvious, that in his time, the *ripeness* was quite equal to what it is at present, as his hypothesis was founded upon the decisive observations of Dr. Heberden, Cavendish, and others, in the year 1766.

VI.—*Constant Voltaic Battery.*

PROFESSOR Daniel, of King's College, exhibited his battery on the 6th inst. at the Royal Institution. He was led to construct this very beautiful apparatus, by following up the investigations of Davy and Faraday. He found that the protecting power of tin on copper sheathing, was due to a chemical action. Thus he placed a plate of silver in a solution of sulphate of copper; and on touching it with a fine-pointed rod of zinc, he found the copper deposited on it in a circular form, and in a regular manner; and, if the contact was kept up, the whole plate was supplied with a copper coating. The effect of protecting metals appeared, at first, an objection to the chemical theory of electricity ; but this experiment demonstrates its truth. To determine and measure the definite chemical action of electricity, Mr. Daniel has constructed a *dissected battery*. It consists of ten cylindrical glass vessels, which contain the fluid electrolytes ; the two plates of metal are immersed in these fluids, each plate communicating below by means of a separate wire, which is made to perforate a glass stopper closing the bottom of the cell, with a small quantity of mercury contained in a separate cup below the stopper. The plates consisted of amalgamated zinc and platinum ; the electrolyte consisted of 100 water, and 2·25 sulph. acid. He found that by increasing the size of the platinum plates, the action was promoted, and that the zinc might be reduced to the size of a wire, with the same effect as when a plate was used. Iron answers in place of the platinum, but not instead of zinc. The dilute acid described, has little action on the amalgamated zinc, because the latter becomes speedily covered with bubbles of hydrogen, which mar its action.— When nitric acid is added, the plate is soon dissolved, without ex-

.tricating any gas, in consequence of the elements of the nitric acid combining with the nascent hydrogen. Nascent hydrogen also de-oxidates copper. To remove the hydrogen, he constructed the *constant battery;* which consists of a copper cylindrical vessel, containing in its axis, a membranous tube formed of the œsophagus of an ox, in which is suspended a rod of zinc. Dilute acid is poured into the membranous tube, by means of a funnel; and passes off by a syphon, communicating with the bottom. The space between the animal tube and the sides of the copper cylinder, is filled with a solution of sulphate of copper, and pieces of this salt, to keep the solution saturated. By this arrangement, the oxide deposited is removed as it is formed, by the syphon tube; and the hydrogen evolved from the surface of the copper, is absorbed. For, on completing the circuit, the electric current passes freely through the blue vitriol solution, and no hydrogen appears on the conductor; but the latter is covered with a coating of pure copper. The advantages of this battery are obvious; it may be kept for hours in action, with the same power, and is economical.

VII.—*Pharmacy, &c.*

1. *Quassin.* — Winckler has succeeded in obtaining the bitter principle of the *quassia amara* in its pure crystalline state. He prepares it by digesting 3 ounces of pulverised quassia wood in 2 lbs. spirit of wine of 80 per cent. evaporating the tincture in a water bath, dissolving the remainder in water, filtering the solution; he then evaporates it in the water bath to the consistence of a thick extract, and treats it with water and spirit of 80 per cent., with small portions of absolute alcohol as long as they take up a bitter taste. The spirituous tincture is then filtered, evaporated in a water bath, the dry residue treated with hot water, when a small quantity of dark brown matter remains. The filtered solution possesses a yellow-wine colour; it should be decolourized by animal charcoal, and evaporated at a gentle heat; the *quassin* separates in fine white prisms. From the watery extract no crystals can be obtained, but merely a yellow-deliquescent mass. Quassin is soluble in water, more so in spirit, very little in ether. By diluting the alcoholic solution the *quassin* is obtained in the form of a woody mass. The aqueous solution is precipitated white by tannin and corrosive sublimate.—*Central blatt, Jan.,* 1836, 69.

2. *Chinese Rhubarb.*—For the discovery of the true plant which supplies this drug, (usually called Indian), the Russian government have for several years offered a reward of 30,000 roubles. M. Paravey, among some Chinese works which he has examined at Paris, found two figures of the plant with violet and white flowers, shewing that the source of this medicine is not confined to the *Rheum palmatum R.* and *R. undulatum.*—*L'Institut.,* 150.

3. *Rapid mode of preparing Mercurial Ointment.*—According to Van Mons mercury can be rapidly killed by adding some drops of *Balsamum sulphuris terebinthinatum. — Buchner's Repertorium,* iv. 272.

4. *Extract and Tincture of Rhubarb and Extract of Gentian.*
—It is difficult to clarify the aqueous extract of rhubarb. Geiseler recommends digesting the entire root of the rhubarb with water, and setting it aside in a close vessel exposed to the action of the steam. The extractive parts dissolve completely, the solution becomes clear, and the root consists only of fibres. The specific gravity of the preparation thus obtained is 2·048, and a pound of *Rheum muscovit.* gives 8½ ounces of a soluble extract of the consistence of pills. In the same way he has prepared extract of gentian. Six pounds of *Rad. gentian.* gave four pounds of a dark yellow soluble extract.—*Central blatt, Feb.*, 1836.

5. *Infusion of Rubarb.*—The roots cut down are to be placed in the carbonate of potash, in the requisite quantity of cold distilled water. The vessel should then be introduced into a steam apparatus, and allowed to digest at the temperature of 189°½. It should then be filtered, and after the addition of cinnamon water, placed in a cool situation. This infusion contains very little starch, and will keep longer than when prepared with boiling water.—*Ibid.*

6. *Tincture of Broomseed* is recommended by Dr. Pearson in doses of one or two drachms as a diuretic in dropsy. It is prepared by digesting for ten days two ounces of the bruised seeds in 8 ounces of spirit. If it produces diarrhea, a few drops of tincture of opium may be given with each dose. Dr. Pearson considers the broom superior to digitalis and squill from its improving the appetite and invigorating the whole system. It is not adapted, however, to hydrothorax attended with thoracic inflammation, nor to ovarian dropsy.—*Brit. and For. Medical Rev., April*, 1836.

VIII.—*Observations of the Planet Jupiter.*

THE following observations of this planet were made by Mr. T. G. Taylor, Honourable Company's Astronomer, at Madras Observatory, March 6, 1835. Occultation of 50 Tauri behind the moon's dark limb, at 9h. 19m. 11·06s. disappearance instantaneous; observed with 5 feet Achromatic power 60.

March 6, 1835. Occultation of Jupiter by the moon's dark limb.

At 10h.	6m.	40s.	M. T. The moon's border had slightly impinged upon the limb of the planet.
10	7	7	The moon's border, as well as could be judged, had reached the centre of the planet; but the figure of the latter was so distorted that the observation cannot be depended upon to 5 seconds.
10	8	17·88	The planet was totally occulted; observation correct to one-tenth of a second.

Barometer, 30·118; light breeze from the S. E.; air hazy.
Thermometer, 81·2.
Wet bulb ditto, 76·8.
During the latter part of the observation, the distortion con-

tinued exhibiting at 5 seconds before the total occultation, a thread
of light, three quarters of the diameter of the planet in length, and
much more obtuse at the apex, than the ordinary figure of Ju-
piter.

The occultation of the fourth satellite, which was carefully
watched, could not be observed, in consequence of its extreme faint-
ness. When within 1' or 2' of the moon's border, it disappeared se-
veral times altogether; and after 3" or 4" again became distinctly
visible. The other satellites, during these intervals, did not un-
dergo the slightest change in brilliancy; but on arriving within 2'
or 3' of the moon's border, the same phenomena occurred.

From these observations, it appears highly probable, that the
moon *is* surrounded by an atmosphere; and that its extent is not
less than that of our earth. The nature of the observation, how-
ever, (which is only prolonged during a few seconds) leaving no time
for consideration, or measurement, necessarily leaves us in a state of
doubt. It is highly important, and deserves attention.—(*Madras*
Journal of Lit. and Science, ii. 165.)

IX.—*Solar Ecclipse on the 15th May.*

As it seemed a point of some interest to determine the gradual in-
crease of solar radiation during the diminution of the obscuration. I
kept a register of the Thermometer, which was exposed in a favour-
able situation to the action of the sun. The following table contains
the result commencing at 3h 41' or 22·'5 after the middle of the
eclipse. The Barometer was also exposed to the sun, and is corrected
for expansion.

		Ther.	Bar.
3h	41m	66°F.	
3	45	70·5	
3	48	71·5	
3	50	72	29·98 inches
3	53	73	
3	55	74	
4		74·5	
4	3	75	
4	6	76·5	
4	10	77	29·747
4	15	77·5	
4	19	77	
4	27	78	29·714
4	30	77·5	29·725
4	38	78	29·694
4	39	78·5	29·679
4	44	80	29·669
4	48	80·5	29·658
4	50	81·5	29·644	Edit.

X.—*Footmarks of unknown Animals and Birds in New Red*
Sandstone.

Our geological readers are familiar with the description, by Dr.
Duncan, of the traces of animal impressions in the new red sand-

stone of Dumfries-shire. Traces of unknown animals have recently been detected in a similar rock, at Hildburghausen, in Thuringia, by M. Lickler. Traces of four species of different animals can be observed. Two footmarks are always found together; one behind about six inches long, the other before, only half as large. The toes are five. The large toe is situated at a right angle in relation to the others. The two large toes of one pair of feet are directed always from the same side, but the same toes of the following pair are directed in the opposite way. The animal must, therefore, have ambled. A remarkable feature is, that the pairs of feet follow in a right line:— hence the animals must, when they walked, have raked the earth. Count Munster considers them to have been amphibia; Weiss, on the contrary, mammiferae; while Link believes them to have been gigantic sauri, like the chameleon.—(*Bibliotheque Universelle,* 1835, vol. ii. 399.)

The first traces of birds, however, in a similar situation, have been discovered on the banks of the Connecticut river, in Massachusetts; and described by Professor Hitchcock, of Amherst College. The appearance presented is that of the feet of a bird which had been walking in the mud. The depressions are more or less perfect and deep; and have been made by an animal with two feet, and usually three toes. In a few instances a fourth, or hind toe, can be observed, not exactly in the rear, but inclining somewhat inward; and in one instance, the toes all point forward. Sometimes these ternate depressions run into one another, as the toes approach the point of convergence, but they also sometimes stop short of that point, as if the animal had not sunk deep enough to allow the heel to make an impression. Attached to the posterior impression, there is frequently an appendage resembling a tuft of hairs or bristles. In all cases, where there are three toes pointing forwards, the middle one is the longest. Mr. Hitchcock found these impressions to correspond closely with those formed by small species of recent grallae, particularly snipes. He divides the tracks in the sand-stone into 7 species, under the genus *Ornithichnites.* 1. Pachydactyli; O. *giganteus;* O. *tuberosus;* Leptodactyli; O. *ingens;* O. *diversus;* O. *tetradactylus;* O. *palmatus;* O. *minimus.*—(*Silliman's American Journal,* xxix. 307.)

XI.—*New Minerals.*

Triphylline, (τρις three and φυλη family), from its consisting of three phosphates.. It is described by Fuchs as being crystalline, cleaving in four directions; one of the cleavages is vertical to the others. Two of them are parallel with the sides of a rhombic prism of about 132° and 148°. The primary form is a rhombic prism. Colour greenish gray, in some places blueish; the powder grayish white. In large pieces the lustre is fatty; in thin portions translucent. Specific gravity, 3·6. Hardness nearly that of apatite; fuses readily before the blowpipe. With borax fuses into an iron-coloured glass. It is soluble in acids. It consists of phosphoric acid 41·47; protoxide of iron 48·37; oxide of manganese 4·7; lithia

3·4 : silica ·53 ; water ·68; loss ·65. — *Poggendorff's Annalen,* xxxvi. 473.

Tetraphylline.—This appears to be a variety of the preceding. It was obtained by Nordenskiöld from Keite, in Finland. It contains phosphoric acid 42·6; protoxide of iron 38·6; oxide of manganese 12·1 ; magnesia 1·7; lithia 8·2.—*Ibid.*

XII.—*Anagyris Fœtida.*

THIS tree grows to the height of 8 or 10 feet ; the leaves are ternate, alternate, pubescent below, and supplied with a bifid stipula at their summit. It is indigenous to Greece and the southern, parts of Europe. The bark, according to Peschier and Jacquemin, consists of a fixed oil, chlorophylle, resin, gum, yellow colouring matter, extractive and a peculiar principle. The latter also exists to a considerable extent in the seeds. It is obtained by submitting them, when dried, to the action of alcohol of ·800 with the assistance of heat; and the product of the digestion in alcohol of ·836 ; evaporating the liquor to the consistence of extract, dissolving the matter furnished by the alcohol in water in order to separate the resin and oil and evaporate to dryness. Thus prepared the principle is yellow; its taste is bitter, soluble in water and alcohol.— *Memoires de la Societe de Physique et d'Histoire Naturelle de Geneve,* v. 75.

XIII.—*Action of Gypsum on Vegetables.*

PESCHIER finds that the solution of gypsum which has been removed from the furnace is sometimes acid. 2. That the influence of gypsum has no effect upon vegetables except in solution. 3. That on spreading gypsum upon the leaves during rain, its decomposition is effected in direct proportion to its solution, and the surfaces which the leaves present. 4. That its action upon vegetables is due to the influence which the electric fluid exerts upon them, and upon the chemical combination which they absorb; that from the influence of this fluid the decomposition of these combinations, and the formation of new products depend. Hence, the sulphuric acid is set at liberty and combines with potash in the juice. 5. That the electric influence is equal upon the raw and calcined gypsum. Hence, the former is to be preferred. 6. That the roots like the leaves decompose saline solutions. 7. That hydro-chlorate of lime may be employed with advantage. 8. That the influence of gypsum is not confined to leguminous plants. 9. That when spread upon the leaves gypsum has more influence than upon the roots.— *Ibid.* 180.

XIV.—*Analysis of Morbid Bile.*

PROFSSOR LAVINI found the bile of a woman, aged 34 years, who died of entero-hepatitis to consist of

1. Water forming one half of its weight.
2. Sub-carbonate of ammonia.
3. A resin separated in the form of grains.
4. Carbon in powder.
5. Resin forming part of the liquid.
6. Animal matter of a greenish-yellow colour.
7. The saline matter of healthy bile.
   No albumen.
   No picromel.

It thus differed from healthy bile in containing less water, and in being destitute of picromel and albumen; and also in containing resin and carbon in suspension, as well as sub-carbonate of ammonia produced probably by the alterations of the other principles of the bile, a change which is evinced by the powerful odour of the yellowish green animal matter.—*Memorie della Accad. di Torino*, xxxvi. 200.

XV.—*Phloridzin.*

This substance is prepared by boiling the bark of the fresh root of the apple (the dry root contains very little) during two hours in a quantity of water sufficient to cover the bark. The liquid is decanted and a second decoction made, which is added to the first. Next day a quantity of crystals of phloridzin will have been deposited, which may be purified by treatment with distilled water and animal charcoal. By evaporation to one-fifth the crystals are deposited. By this method 3 per cent. of phloridzin is obtained from the bark. By digesting the fresh bark of the root in weak alcohol at the temperature of 50° for 8 or 10 hours, 5 per cent. may be obtained. It has a bitter taste, crystallizes in white silky needles with a yellowish shade; 1000 parts of water dissolve only 1 part of phloridzin from 0° to 22°. From 22° to 100°, it dissolves in all proportions. Its specific gravity at 19° (66°$\frac{1}{4}$), is 1·4298. At 212° loses all its water of crystallization. When dried at the common temperature, it retains 7 per cent. It fuses at 108° (226°$\frac{1}{5}$), and boils at 177° (350°). At 193° (379·°4), it begins to decompose giving out benzoic acid, pyro-acetic spirit and a brown oil heavier than water. Nitric acid converts it into oxalic acid. Muriatic acid converts it into a white substance. The alkalies and acetic acid dissolve it without producing any alteration in it. Chlorine, bromine and iodine form with it a brown resinous substance soluble in alcohol. Sulphated per-oxide of iron forms a brown precipitate. Per-chloride of iron gives a similiar precipitate. Acetate of lead gives a white precipitate. Chlorine water affords a yellow precipitate. The following are two analyses of it:

	Koninck.	Petersen.
Carbon,	50·905	56·921
Hydrogen,	5·569	5·810
Oxygen,	43·526	37·274

Koninck has employed it with success in intermittent fever in the dose of from 10 to 15 grains. Its combination with the oxide of lead consisted of 57·26 oxide, and 42·74 phloridzin.— *Journal de Pharmacie, Feb.*, 1836.

XVI.—*Examination of the Dung Beetle, (Blaps Obtusa).*

ACCORDING to the experiments of Hornung and Bley, the constituents of this insect are albumen, osmazome with muriate of lime and soda, and phosphate of magnesia, brownish-yellow resin, brownish-red resin, coffee-brown resin, cinnamon-brown resin, mild fat oil, brown colouring matter, animal fibrin with colouring matter, formic acid, uric acid, red colouring matter, wax, animal fibre and water.—*Journal fur praktische Chemie*, vi. 273.

XVII.—*Examination of the Tape Worm, (Taenia Solium.)*

BLEY found this worm to consist of insoluble animal matter with small portions of albumen, resin, fat and ethereal oils, with some osmazome.—*Ibid.*, vi. 271.

XVIII.—*Concretion from the Nose.*

THIS was obtained from the nose of a man who had been affected with syphilis, and had in consequence lost the point of his nose. Herberger obtained from 100 parts, nasal mucus 46·; organic extract by alcohol with lactate of potash and traces of free alkali 3·6; chlorides of potassium and sodium 4·7; organic extract by water and ammonia phosphate of magnesia 14·6; water 29·; impurity 2·; loss 0·1.—*Ibid.*

XIX.—*Diseased Woman's Milk.*

HERBERGER found the composition of this milk which possessed a specific gravity of 1·023; a sweetish taste and peculiar odour without affording butter when agitated; stearine and elain 2·33; casein with ·07 salts of lime 1·835; sugar of milk 2·683; alkaline phosphate with some sugar of milk ·081; lactic acid, chlorides of sodium and potassium and organic matter insoluble in oil of turpentine 3·358; organic tasteless principle reducing chloride of gold, and platinum soluble in oil of turpentine ·166; water and loss 89·542; volatile odoriferous principle with fat *a trace.*—*Ibid.*

XX.—*Analysis of White Blood.*

LECOURT, an old soldier, now employed as a bleacher at Clichy la Garenne, was seized after a party of pleasure with difficulty of breathing and general illness, and next morning vomited a quantity of blood during a fit of coughing. M. Sion, his medical attendant, found it necessaay to bleed him. The blood drawn was milky. In the evening leeches applied, extracted similar blood. It consisted of water 794; albumen 64; acid soap, cholesterine, oleine 117; margarine, stearine-salts and extractive matter 25, with traces of colouring matter. Similar blood was examined in 1830 by Dr. Christison, and in 1831, by Lassaigne, and in 1835, by Zanarelli.—*Journal de Chim. Medic.* i. 402.

Meteorological Journal,

ept at the Manse of the Parish of Abbey St. Bathan's, Berwickshire, Lat. 55° 52' N. Long. 2° 23' W. at the height of about 450 fee above the sea. By the Rev. JOHN WALLACE.

APRIL.

DATE 1836.	THERMOMETER IX A.M.	X A.M.	III P.M.	X P.M.	HYGROMETER (Leslie's) IX AM	X AM	III PM	X PM	BAROMETER at 32° of Fahrenheit IX A.M.	X A.M.	III P.M.	X P.M.	Rain in Inches Weekly	Direction of wind at X A.M.	REMARKS
Full M. Fr. 1	40	40	43	35	10	16	18	3	29·054	29·037	28·952	28·918		W.N.W.	Gentle breeze, cloudy, with occasional snow showers, evening calm.
Sa. 2	35	33¾	37	33¾	2	2	16	10	28·985	28·990	29·127	29·158		N.W.	A.M. brisk wind, snow, P.M. fair, sky unsettled, evening cloudy, wind gusty.
Su. 3	38¼	40	40	33¼	15	20	17	10	29·563	29·845	29·743	29·874		N.WbyN.	Brisk wind, cloudy for most part, evening calm and clear.
Mo. 4	39	39¼	41	34	23	27	28	11	29·890	29·853	29·796	29·757	0·866	N.WbyN.	Gentle wind, A.M. clear & fine, P.M. a thin veil of cirros., evng. calm & clear.
Tu. 5	38½	40¼	42½	40½	1	1	0	0	29·523	29·480	29·396	29·371		S.E.	Gentle wind, A.M. cloudy, P.M. rain, evening foggy, with rain.
We. 6	44½	43¼	48	38½	15	20	33	7	29·343	29·343	29·246	29·078		N.N.W.	Calm, heavy masses of cloud obscuring the sky.
Th. 7	42	43¼	42	37	7	10	9	3	28·694	28·678	28·561	28·568		W.S.W.	Calm, A.M. frequent showers, P.M. fair but cloudy.
Fr. 8	40½	42½	43	57	6	11	11	1	28·657	28·661	28·759	28·809		N.W.	Calm, cloudy and showering, P.M. rain, evening cloudy but fair.
Sa. 9	41	42	41	38½	4	4	1	0	29·053	29·074	29·118	29·166		E.	Calm, with thick fog and tendency to rain.
Su. 10	41½	42	44¼	32½	11	14	19	2	29·201	29·205	29·131	29·236	0·254	N.E.byE	Calm, A.M. cloudy, P.M. gradually clearing, evening clear, deposition.
Mo. 11	38½	42	52½	43½	3	4	7	14	29·170	29·164	29·085	29·050		S.	Hoar frost, P.M. calm, cloudless, P.M. gentle wind, cirros. evng. cloudy, wind
Tu. 12	47	46½	51½	42½	5	10	33	10	28·945	28·937	28·969	29·031		W. by S.	Brisk wind, A.M. cloudy, with showers, P.M. nearly cloudless, aur. borealis.
We. 13	47½	50	45	37½	12	19	27	14	28·858	28·859	28·868	29·093		W.	Gentle wind, cloudy, with light showers, evening foggy.
Th. 14	44½	44¼	44	46	12	15	4	3	29·284	29·303	29·278	29·465		S. by E.	Calm, A.M. nearly cloudless, P.M. large masses of cumulous clouds prevalent
New M. Fr. 15	55	56	61½	46	15	29	34	9	29·583	29·533	29·555	29·548		W.N.W.	A.M. gentle breeze, P.M. sky clouding, P.M. rain with wind northwards.
Sa. 16	52	53¾	53½	39½	13	17	25	5	29·550	29·547	29·561	29·507		S.W.	Calm, cirrostratus, &c. prevalent, P.M. tendency to rain, evening cloudy.
Su. 17	43	44¼	51¼	42	20	23	38	12	29·688	29·686	29·519	29·493		S.W.	Brisk wind, P.M. frequent showers, evening overcast.
Mo. 18	46	47¼	48	40	20	38	23	0	29·500	29·483	29·405	29·442		W.N.W.	Gentle wind, cumuli prevalent, P.M. lowering, in the evening rain.
Tu. 19	44½	45	49¼	43½	28	31	23	10	29·454	29·465	29·413	29·307	0·437	N.WbyW	Brisk wind, soft clouds, a hazy sky, light showers in the evening, wind strong.
We. 20	47½	49½	52½	41	10	13	34	10	29·130	29·103	29·006	29·014		S.W.	Brisk wind, A.M. cirros. on a blue sky, P.M. cirros. polarized from south northw. aurora bor.
Th. 21	44	47	43¾	43	22	30	8	3	29·194	29·192	29·081	28·953		S.W.	Strong wind, a hazy sky, P.M. overcast, showery, evng. calm.
Fr. 22	44½	45¼	50¼	37	22	20	32	0	28·996	28·982	29·019	29·200		W. by S.	Calm, cirri prevalent, A.M. overcast, aurora borealis, deposition.
Sa. 23	42½	44½	40½	33¾	18	24	8	0	29·182	29·158	29·108	29·198		S.W.	Calm, A.M. large masses of cloud floating, evening cloudless.
Su. 24	41	41¼	47¼	38	9	11	38	12	29·413	29·413	29·467	29·514	0·246	N.	Brisk wind, cumuli over a blue sky, evening clear and calm.
Mo. 25	48¼	49½	50½	39	22	22	32	8	29·473	29·464	29·450	29·479		N.	Strong wind, A.M. sleet showers, in the evening light clouds over a clear sky.
Tu. 26	48½	45¼	45¼	36¼	27	31	32	8	29·350	29·346	29·241	29·321		W.N.W.	Brisk wind, cumuli over a clear sky, P.M. cloudy, frequent showers, evening calm.
We. 27	41	41¼	44¼	40	11	13	19	5	29·486	29·508	29·548	29·554		N.N.W.	Brisk wind, floating cumuli, evening nearly cloudless.
Th. 28	39½	40¼	39	30	9	16	14	9	29·300	29·318	29·344	29·410		N.W.	Brisk wind, masses of floating cumuli, evening nearly cloudless.
Fr. 29	35½	36¾	42½	35	24	29	40	17	29·344	29·344	29·278	29·304	0·007	N.W.	Brisk wind, masses of cumuli, evening clear.
Sa. 30	40½	39¼	42½	38½	17	19	19	10	29·275	29·286	29·303	29·343		N.W.	Brisk wind, cloudy, with occasional hail showers.
Means for the Month				38·3	14	18	22	7	29·270	29·268	29·246	29·282	1·810		

Means for the Month — Therm. 40·6. Hygr. 13. Bar. 29·...

Mean temperature of spring water 46°?
Mean point of deposition 40·?

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END OF VOL. III.

ADDITIONAL ERRATA IN Vol. II.

Page 264, line 17, before common, insert not
   „ 266, last line, for powder, read power
   „ 270, line 11, for chlorate, read chlorite
   „ 342, „ 31, ib.
   „ ib. „ 33, for mixed, read unmixed
   „ 431, „ 23, for hypo-sulphuret, read poly-sulphuret
   „ 433, „ 18, for hypo-sulphate, read hypo-chlorite
   „ ib. „ 20, for chlorate, read chlorite
   „ 464, „ 30, for 920, read 9·20
   „ 465, „ 1, for 116·75, read 126·8
   „ ib. „ 2, for 965, read ·905
   „ ib. „ 4, for 1247, read 12·47
   „ 478, „ 21, for fluxivoal, read fluxional

ERRATA IN Vol. III.

Page 91, line 2 from bottom, cancel not
   „ 224, „ 8, „ for woolton read motion
   „ 300, „ 9, „ for bromide read iodide